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ORIGINAL ARTICLES.

I.—On a New Specimen of the Mesosaurian Reptile, *Stereosternum tumidum*, from San Paulo, Brazil.

By Arthur Smith Woodward, F.L.S., F.G.S.,
Of the British Museum (Natural History).

(PLATE V.)

WHEN in Rio de Janeiro last October, Dr. Orville A. Derby gave me for publication in the Geological Magazine the photograph of a fine new specimen of *Stereosternum tumidum*, reproduced in the accompanying Plate V. Like the type-specimens discovered by Dr. Derby, it was obtained from a light-grey limestone in a quarry on the estate of Senhor Antonio Machado de Campos, near the town of Limeira, in the State of San Paulo, Brazil. It is interesting as showing for the first time the general proportions of the trunk and tail of this strange extinct reptile; and it also permits the identification of another fossil from the same formation, now in the British Museum, which has hitherto escaped notice.

Stereosternum was originally described by Professor Cope in 1886[1] from an imperfect trunk and other fragments, which he could not assign to a definite systematic position, but believed to represent an aquatic reptile very similar to the small *Mesosaurus* from the Karoo Formation in South Africa. A year later he received from Dr. Derby a "nearly entire specimen," showing the head, neck, and fore-limbs, and this was the subject of a brief note in the "American Naturalist."[2] Professor Cope remarked:—"The neck is moderately elongate, and includes nine vertebræ besides the atlas. The vertebræ have robust transverse processes and slender cervical ribs. The skull is elongate and acuminate, and its bones are apparently fragile. Little of its structure can be made out. The teeth are slender, acute, and nearly straight, and are planted in (?) shallow alveoli." About the same time Dr. Baur[3] emphasized the

[1] E. D. Cope, "A Contribution to the Vertebrate Palæontology of Brazil": Proc. Amer. Phil. Soc., vol. xxiii (1886), p. 7, pl. i.

[2] E. D. Cope, "The Carboniferous Genus *Stereosternum*": Amer. Nat., vol. xxi (1887), p. 1109.

[3] G. Baur, Zool. Anzeiger, 1886, p. 189, and "On the Phylogenetic Arrangement of the Sauropsida," Journ. Morphology, vol. i (1887), p. 103.

importance of the structure of the tarsus, which exhibits a separate tarsal bone supporting each of the five digits, and made *Stereosternum* the type of a new reptilian order, termed Proganosauria. Other specimens wanting the greater part of the neck and tail were acquired by the British Museum, and these were noticed by Mr. Lydekker in 1889,[1] described by Professor Seeley in 1892.[2] It is now evident that the animal is related in some undetermined way to the ancestry of the Plesiosauria.

The new specimen of *Stereosternum* is shown in Pl. V of about half the natural size, and is curled up on the slab of limestone so that the end of the snout touches or overlaps the slender extremity of the tail. The head and neck are only indicated in impression; but it is clear that the head is of elongate-triangular form, with extremely delicate bones, and if the tip of the snout be truly represented by the end of the impression near the recurved tail, the length of the head is much less than that of the neck. Although the hindermost cervical vertebræ are proved to be short by one of the British Museum specimens, the impression seems to show that the centra of those nearer the head are considerably longer than deep. The skeleton is sharply bent and imperfect in the region of the pectoral arch; but the number of vertebræ between the neck and the sacrum must be about twenty, as already determined in a British Museum specimen, while the series of ribs is well shown on one side. The remains of the hind-limbs correspond with those of previous specimens. Assuming that an obscure fragment of the pelvis overlaps the transverse processes of the two hinder sacral vertebræ, the tail comprises not less than sixty vertebræ, of which at least the foremost seven bear robust transverse processes gradually diminishing in size backwards. The centra of the first twelve caudals are comparatively short and stout and exposed from above or below; the following thirty-five vertebræ, which are more elongated, are shown from the side, with the neural arch projecting within the curve of the tail, and the delicate loose chevrons lying parallel with the long axis of the centra on the outer side of the curve; the remaining vertebræ, to the number of about fourteen, are very slender and indicated only by an obscure impression. As a whole the tail is thus somewhat more than twice as long as the trunk, occupying slightly less than three-fifths of the entire length of the animal.

A detached tail of a precisely similar character, discovered in the same stratum and locality as the portions of trunk of *Stereosternum* in the British Museum (i.e. Ipanema), is interesting as showing not merely the skeleton but also an impression of the soft parts. This specimen (Brit. Mus. No. R. 538) is 0·13 m. in length and displays forty vertebræ in regular series, those towards the hinder extremity

[1] R. Lydekker, "Catal. Foss. Rept. Brit. Mus.," pt. ii (1889), p. 302. Though the locality is not mentioned in the Catalogue, the dealer's labels state that these specimens were obtained from Ipanema.

[2] H. G. Seeley, "The Mesosauria of South Africa": Quart. Journ. Geol. Soc. vol. xlviii (1892), pp. 598–602, figs. 2–4.

only feebly ossified. All the centra are longer than deep and a little constricted; while some of them clearly exhibit the segmentation into two parts already noted in the anterior caudal vertebræ of previously described specimens. The backwardly inclined short neural spines are distinct, except in about six terminal vertebræ; and there are traces of the slender chevron bones as far as the thirteenth vertebra from the end. The soft tissues are indicated by a brown stain, of which the border is very sharply defined below, but less continuously observable above. At the thirtieth vertebra from the end the depth between the two borders of the stain is only 0·008 m., while at the terminal vertebra the depth is 0·005 m. Unless appearances are deceptive, and this is improbable, the tail of *Stereosternum* must thus have been an excessively slender appendage, without any dermal expansion at the end.

A fragmentary detached maxilla, from the same limestone and the same quarry as the type-specimens of *Stereosternum*, exhibits a few very long and slender teeth, like those already observed by Cope in this genus. This fossil is in the San Paulo Museum.

It is also interesting to add that Dr. Derby has obtained a typical Labyrinthodont tooth from a siliceous limestone at Conchas, which is almost certainly on the same horizon as that at Limeira. In fact, evidence is gradually accumulating to render it still more certain that the Karoo Series of South Africa is well represented by homotaxial deposits in the south of Brazil and in parts of the Argentine Republic.

EXPLANATION OF PLATE V.

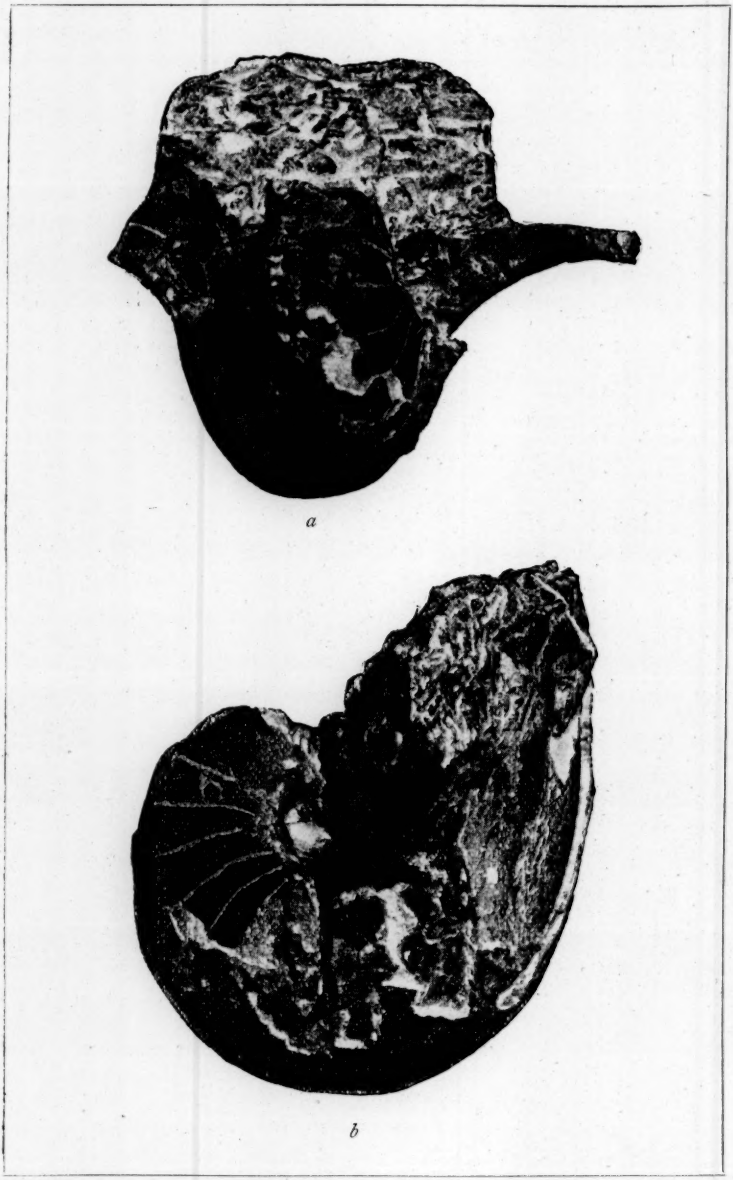
Stereosternum tumidum, Cope; photograph of skeleton, about one-half natural size. Early Mesozoic: near Limeira, San Paulo, Brazil. [Original in Museum of San Paulo, Brazil.]

II.—On a New Genus and Species of *Nautilus*-like Shell (*Acanthonautilus bispinosus*) from the Carboniferous Limestone of Ireland.

By Arthur H. Foord, Ph.D., F.G.S.

(PLATE VI.)

THE shell to which I have given the above name is at present unique. It was found, a few years ago, in a quarry at Clane, in the county of Kildare, where I had for some time been in search of Cephalopod shells. It was associated with characteristic species of Carboniferous Limestone fossils, among which the following Nautiloid shells may be mentioned, viz. *Solenocheilus dorsalis*, Phil., sp., *Asymptoceras cyclostomus*, Phil., sp., *A. Foordi*, Hyatt, and an undescribed species allied to the last-named one. The marginal spines, at first obscurely seen in the matrix, at once attracted attention; and although one was broken off and lost in the quarry débris, the other was fortunately found in the fragment of rock struck off from the shell, and was then carefully developed and attached to the latter. Naturally very diligent search has been made since its discovery, and a sharp look-out kept for any further specimens of this interesting species, but so far without any result.



a

b

Acanthonautilus bispinosus, A. H. Foord.
Carboniferous Limestone: Clane, Co. Kildare, Ireland.

a. Front view, showing spines and sutures.
b. Side view, showing sutures and general outline. } $\frac{3}{8}$ nat. size.

Description.—The shell is of medium size, *Nautilus*-like in its general habit, somewhat globose, and expanding rapidly. It consists of about two or two and a half volutions, the inner ones of which are concealed by the lateral expansion of the shell. The peripheral area is broadly rounded and somewhat flattened on the body-chamber, especially towards the aperture, in the adult, but more narrowly and evenly rounded in the younger stages of growth. The umbilicus is rather large and very distinctly funnel-shaped. It is provided with a thick and very conspicuous rim, which is produced into long, flat, hollow, spine-like processes, projecting almost at right angles to the longitudinal axis of the shell on each side of it. The spines, whose flatness is nearly on the same plane as the surface of the peripheral area, are greatly expanded at their base, but become rapidly narrow towards their distal end, their basal portion merging with a slight concavity in the general surface of the abdominal part of the shell on the outer side, while on the inner side they are continuous with the umbilicus. The spines are met anteriorly by the border of the mouth; posteriorly by the rim of the umbilicus, from which they proceed on that side. They thus make here a curve on their inner edge which is directed outward and slightly downward at the same time. The direction of the spines with reference to the longitudinal axis of the shell is nearly that of a right angle, with a perceptible inclination downwards, that is, towards the septate part of the shell.

Both the spines are imperfect, one having been broken off near the base, the other at a distance of about 50 mm. therefrom. The former is seen to begin with two diverging folds of the shell, originating as above described. These make an angle, of which the apex is formed by the rim of the umbilicus. The two folds rapidly approach each other, till, as the more perfect spine shows, they run nearly parallel, forming a flattened tube up to the end of the spine, as far as it is preserved. The flattening may be partly due to pressure in the rock, but, on the other hand, the pinched look of the two folds of the shell near the base of the spine, where there is no evidence of crushing, is already so marked as to contradict the supposition of an artificial flattening of the spine in a great measure.

The outline of the aperture, which can be accurately traced, is curved gently forward on either side, and forms in the centre a broad and shallow hyponomic sinus.

The sutures, of which only five are visible, are moderately distant, the last two somewhat closer than the rest. The septa have been completely destroyed by the deposition of crystalline calcite in the chambers, the removal of a great part of which revealed no traces of them; nor were any remains of the siphuncle found.

The test is quite smooth, only slightly marked lines of growth occurring on the body-chamber. It is about 1 mm. in thickness.

The greatest diameter of the shell is 16 cm., measured from the outer margin of the aperture to a point diametrically opposite to this. The transverse diameter, measured across the shell from the base

of one spine to the base of the other, is 13·5 cm. The distance between the septa, measured from the posterior to the anterior, is 14 mm., 17 mm., 20 mm., 13 mm., and 12 mm.

Remarks.—The presence of the long, marginal spines dissociates this form from any other Nautiloid shell known to me, and dictates its generic appellation, while it also constitutes its chief specific character.

In seeking for species related to the present one in their general form, those comprised under the genus *Solenocheilus* (Meek and Worthen), and its congeners *Lophoceras* (Hyatt) and *Asymptoceras* (Ryckholt, emend. Hyatt), suggest themselves as being the nearest to it. In some of these species there arise from the angles of the margin of the aperture spout-like projections which at first sight might be taken, in their imperfect condition, for the stumps of spines similar to those of the present species. Such appendages are seen in a species described by Meek and Worthen[1] under the name of *Nautilus* (*Solenocheilus*) *Leidyi* (*Lophoceras Leidyi* of Hyatt).[2] This species (which was found in the Keokuk beds at Warsaw, Illinois), consisting only of the body-chamber, has on each side of the shell, at the junction of the lateral margin and the umbilical wall, a "kind of pinched prominence," which "seems even to have projected outward somewhat as in *Argonauta gondola* of Adams,[3] though probably not to the same extent." It is clear that the spout-like projections here described correspond in no way, except in their position, with the long, flat spines of *Acanthonautilus bispinosus*. The former may, like the hyponomic sinus of the aperture, have given exit to some of the tentacular appendages; the latter, not being channelled, could have served no such purpose. What purpose they did serve it is difficult to conceive, but it is safe to say that they furnished, in a measure, a safeguard against attack.

The thickened rim of the umbilicus is paralleled only in one other Nautiloid, so far as I know, viz. the *Nautilus terebratus* of Dumortier[4] from the Upper Lias of La Verpillière (Isère), France, and of Lincoln. The rim of the umbilicus in this species ends in a slightly prominent lateral angle.

As the spines in the present species are perfectly symmetrical, it is obvious that they were secreted by symmetrical organs. What these organs were, must be matter of pure conjecture. It cannot be supposed that they were of the nature of the tentacles of the *Nautilus* or of any of the cuttle-fish tribe now existing. There is reason to suppose, however, from their hollow structure that the spines were secreted from the inside.

[1] Geol. Surv. Illinois, V, Geol. and Palæont., 1873, p. 524, pl. xviii. figs. 2, 2*a*.

[2] Geol. Surv. Texas, Fourth Annual Report, 1892 (1893)—Carboniferous Cephalopods (second paper), p. 466.

[3] For a figure of this see Lovell Reeve in Conch. Iconica, vol. xii, 1860, pl. iv, fig. 3. I have not access to the original source of the species.

[4] Études Paléontologiques sur les Dépôts Jurassiques du Bassin du Rhône, pt. iv (Lias Supérieur); see also Catalogue of Fossil Cephalopoda in the British Museum, pt. ii, 1891, p. 204, ff. 36, 37.

III.—Notes on a Collection of Rocks from the Tonga Islands.

By J. J. H. Teall, F.R.S., and E. T. Newton, F.R.S.

(Communicated by permission of the Director-General of the Geological Survey.)

A SMALL series of rocks collected at several localities in the Tonga Islands by Lieutenant J. A. Waugh, R.N., during the voyage of the "Penguin" in 1895, has been forwarded to the Geological Survey for examination, by Admiral Wharton, R.N., F.R.S., Hydrographer to the Admiralty. As in some particulars these rocks supplement those already described by Mr. A. Harker (Geol. Mag. 1891, p. 250), from the same group of islands, it seems desirable that a short account of them should be placed on record.

The specimens first to be noticed were obtained from Mango Island, one of the most central of the Tonga or Friendly Islands, where some of Mr. Harker's rocks were also collected. It appears from the information supplied with the specimens, that at the south-eastern extremity of Mango Island there is a series of small cliffs, and from these, at a height of six feet above high-water, three types of rock were collected.

No. Ia (F. 311). This is a fine-grained and bright-green, indurated mud, which does not easily disintegrate in water. Under the microscope the rock is seen to be largely composed of irregular, and often angular, fragments surrounded by narrow, deep-green borders. It is possible that these fragments may represent lapilli of volcanic glass, and that the green substance may be of the nature of palagonite; but the evidence for this is by no means satisfactory.

No. Ib. Fragments of light-yellowish and reddish decomposed andesitic tuffs.

No. Ic. This is a creamy-white, fine-grained, and chalky-looking rock, similar in appearance to that noticed by Mr. Harker (loc. cit., p. 255); it is very calcareous, and when examined as powder, in water, under the microscope, is found to contain numerous sponge spicules; but the greater part of the rock is made up of amorphous calcareous particles.

No. II. This specimen was likewise obtained from Mango Island; but from its highest point, 139 feet above high-water; it is a coarse, decomposed, andesitic agglomerate.

Both the green mud (Ia) and the white rock (Ic) appear to have been deposited under water, and although they are still only six feet above high-water, it is probable that they have been upheaved since their deposition.

No. III (F. 312). Two specimens from Tanoa Island, north-east of Mango, were obtained at 20 feet above high-water. One of these is a pale-brown andesitic tuff, and a microscopic section of it shows crystals of green augite and felspar, but evidently mixed with "coral sand," for it includes many fragments of organisms, such as corals, sponge spicules, foraminifera of several kinds, and possibly Radiolaria. Among the foraminifera, *Globigerina* seems to be the most abundant; but the first to attract attention, even in a hand-specimen, are some spherical forms (a large one is 6 mm. in

diameter) which, on comparison with examples of *Orbulina universa*, kindly lent by Dr. G. J. Hinde, leave no doubt as to their belonging to that species.

The shells of the foraminifera are now represented by a colourless isotropic substance, presumably silica, the whole of the carbonate of lime having been removed.

No. IV. From the centre of Telekitonga Island, one of the Otu Tolu group, rounded lumps of pumice, which float in water, were collected at about 10 feet above high-water. These are composed of crystals of hypersthene, augite, and felspar in a highly vesicular, nearly colourless glass. This is the type of rock so widely distributed throughout the Pacific region.

No. V (F. 313). This is a rather coarse and partly indurated "coral sand" from Lalona Island, another of the Otu Tolu group; it is said to be a few feet above high-water, and a similar rock occurs round the shore-line of all four of the islands forming this group. It is a fairly hard rock containing rolled fragments of corals, nullipores, and shells of small molluscs. A few examples of the large stellate foraminifer, *Tinoporus baculatus*, may be seen. This Rhizopod is common in the South Pacific, and occurs in some abundance in No. VIII specimen. Professor Sollas found that *Tinoporus* and *Orbitolites* form the greater part of the "sand" which fills many of the interstices of the coral reef at Funafuti (see *Nature*, Feb. 18, 1897).

No VI is a pumice of hypersthene andesite similar to No. IV, from the middle of this island of Lalona, at about 10 feet above high-water.

No. VII (F. 314). A consolidated "coral sand" similar to, but finer than, No. V, from the eastern side of Lifuka Island, one of the Haapai group, just above high-water. Rolled fragments of corals, nullipores, etc., make up the mass of this rock, which, however, is remarkable for the great number of the foraminifer, *Tinoporus baculatus*, it contains, accompanied by the very similar, but spineless form known as *Gypsina*.

No. VIII (F. 315). Dark-brown sandy tuff, composed of andesitic lapilli and broken crystals of hypersthene, augite, and felspar, from Mo'unga'one Island, Haapai group, 30 feet above high-water.

No. IX is from the same island, but at the coast-line and from rocks 12 feet high. This is a cavernous mass of "coral rock," very dense and much weathered, consisting almost entirely of indurated coral, and may well have formed part of a coral reef. If the mass of the rock is similar to this specimen, it is probable that it is an elevated coral reef.

No. X (F. 316) is a portion of the beach rock of Lofanga, another of the Haapai Islands, and is said to have dead corals still adhering to it. This, again, would seem to indicate an elevation of the shore. The rock is an andesitic tuff; both lapilli and crystals are surrounded by a thin incrustation of calcite, which acts as cement and serves to bind the particles together.

It is interesting to note that even in so small a series of specimens

as those above noticed, there is evidence of some of these Tonga Islands having been upheaved, for although the consolidated "coral sand" may have been thrown up to its present level by the sea, and have been indurated very recently in that position, yet it seems highly probable that some of the rocks (Ia, Ic, III, and possibly IX and X) have been formed under water and afterwards elevated.

Mr. Harker, in concluding his paper, speaks of the possibility of some of the rocks of the Tonga Islands proving eventually to be of Tertiary age. None of the specimens we have examined give any evidence of such an antiquity.

IV.—On the Erratic Boulders and Foreign Stones in the Drift Deposits of Eastern England, and their Lessons.

By Sir Henry H. Howorth, K.C.I.E., M.P., F.R.S., F.G.S.

(*Concluded from the March Number*, p. 130.)

FOLLOWING in the wake of that admirable institution the Boulder Committee of the British Association, whose work ought to have been done by the Geological Surveyors, we may divide the districts from which the foreign boulders (that is, boulders extraneous to the immediate district) have come to Eastern England into four, namely, Charnwood Forest, the Hills of Derbyshire, Durham and Cleveland, and lastly Norway.

First, in regard to Charnwood Forest. Nearly all the boulders which have travelled thence have travelled to the south-west, south, south-east, or east, a small number only having been found to the north of the parent rock.

This district, where a considerable mass of crystalline rocks is exposed, is particularly fitted to test the problem of local glaciers in South-Eastern England. It is very curious that, with the exception of a very small patch of smoothed rock found by Mr. Harrison, not a trace of glacial action can be found here. Upon this Professor Carvill Lewis is emphatic, and his conclusion is supported completely by two such good observers as Professor Bonney and Mr. Hill. They also support Mr. Lewis in attributing to other than glacial action the local patch referred to by Mr. Harrison, which it must be remembered is only about two yards long. Professor Carvill Lewis, an extreme glacial champion, says of it that it is not an example of true glacial striæ, but is a very local smoothing due to moving gravel or possibly an iceberg ("Glacial Geology of Great Britain," p. 61).

This conclusion is very important, for several reasons. If there were no ice on the high ground of Charnwood Forest, which is over 800 feet high, how can we believe in its existence on the Lincolnshire and Norfolk Wolds, which are in the same latitude, and very much lower? This is another argument to add to those I used in a former paper with regard to the impossibility of understanding the former existence of local glaciers on these Downs. But a more important fact remains. If there are no

traces of glaciation in the mountainous region of crystalline rocks of Charnwood, where such traces would be so easily preserved, proving, in fact, that no ice existed there or passed over the district, how can we account for the distribution of the Charnwood Forest boulders by ice at all? This, in itself, conclusive argument does not stand alone. If the glaciers were local, how comes it that nearly all the erratics from this district are found to the south of their place of origin and are not distributed all round it in an equal degree? If the ice came in from the north, how could any of the boulders travel north at all in the face of it? Again, inasmuch as these boulders from Charnwood Forest are often mixed with those from Derbyshire and elsewhere in the beds where they are found, how can ice have brought any of the stones? It could not bring stones from the north, carry them across the Charnwood Forest area, and mix them with Charnwood Forest stones from Mount Sorrel and elsewhere, without leaving substantial evidences of its passage in Charnwood Forest itself.

Putting Charnwood Forest aside, we have to explain how the Mountain Limestone and Millstone Grit of Derbyshire, the Carboniferous rocks of Durham, the basaltic rocks from the dykes of Durham and Cleveland, come to be mixed as they are mixed in almost every part of Eastern England where so-called glacial beds occur. How could the ice from the north-west come down to Finchley and to Norfolk without stopping the passage of the ice from Durham across the same path? I confess I do not know; nor do I know how, when this ice got to its destination, it could begin mixing the ingredients of the chalky clay, which came from different parts of heaven, and redistribute them as we see them distributed. The theory is too fantastic and ridiculous for words, and the fact that it has so many adherents is a continual reproach to nineteenth-century science. I have not yet done. I have referred above to the Norwegian boulders. Upon them I had a polemic in these pages some time ago with Mr. Harker and others. Feeling it to be impossible to attribute their portage to ice in any form, I ventured to suggest as a possible alternative that they had been brought to where they are found as ballast, or they might possibly be Scotch rocks. In regard to the former alternative, I found after I had written what I did that Mr. Clement Reid, a very careful observer, but with many of whose inferences I cannot agree, had warned geologists of the great quantity of such ballast that had been imported into Eastern England from Russia and Norway, and had been distributed over its fields; while Professor Hughes had pointed out several cases of wrecked ships on the Eastern coasts, where large numbers of Norwegian boulders had found their way to English beaches.

I am bound to say that, while giving due weight to these considerations, the reports of the Boulder Committee of the British Association, and especially Mr. Sheppard's recent researches, make it impossible for me to maintain any longer the extreme position I took up in my controversy. I am convinced now that the rocks are

certainly Norwegian, and that these Norwegian boulders have actually been found in undisturbed Boulder-clay and in its associated beds, and that, therefore, if they are to be explained, some other explanation than the one I gave must be forthcoming. My present object is not to point out what that cause was, but to show that it was not, and could not be, ice. Those who have invoked an ice-sheet in the North Sea, and have thus explained the portage of the Norwegian stones, so far as I know have done so, firstly, in order to account for those stones; secondly, to account for the diversion of the Durham drift to the south, it being thought that only a barrier of ice in the North Sea would have diverted the supposed ice of the Tees, with its burden, to the southward; and thirdly, to account for the peculiar glaciation of the Shetlands. This theory of the North Sea ice-sheet has had some vicissitudes in its history, and I have had a good deal to say to it on other occasions. At first it was made to occupy the whole North Sea area, extending as far as the five-hundred fathom line. Then Professor James Geikie went to the Faroes, and found that those islands had had a local set of glaciers of their own, so that it was impossible to believe that the great ice-sheet had extended over them. It was still, however, maintained to have existed on a great scale, and made to overwhelm the Shetlands, although not a trace of one undoubted Norwegian boulder had ever been found in those islands; while Mr. Milne Home, a most experienced Glacial geologist, and Chairman of the Scotch Boulder Committee, continually pressed the conclusion, as it seems to me unanswerably, against the Geological Surveyors that the striæ pointed in a direction inconsistent with such an ice-sheet. Then came the observations on the Lofoden Islands, showing that they are pinnacled and unweathered, and therefore that no ice-sheet could ever have overrun them; then came Pettersen's famous examination of the question from the Norwegian side, showing that the ice from Western Norway, far from having traversed the North Sea, had not even reached the line of islets that run down that coast. This conclusion, which I have enlarged upon in my "Glacial Nightmare," has been extended to South-Western Norway by De Geers, whose work is much relied upon by Dr. James Geikie, and who says that in the second Ice Age (that is, with him, the great one) it is improbable that the land-ice traversed the deep channel which now bounds the coast of South-Western Norway.

Dr. James Geikie, who was with Croll the joint prophet of the original North Sea monster, has, in the last edition of his "Ice Age," made a great concession to the enemy on this question. Speaking of the glaciation of Scandinavia, he now says: "Along the west of Norway the great fiords were at the same time occupied by glaciers, *which do not appear to have extended beyond the general coast-line, but carved their icebergs there*" ("Great Ice Age," p. 46. The italics are mine). For this conclusion the authority of Pettersen and De Geers is quoted, and it is a very just one. How it is possible in any way to reconcile it with Professor Geikie's two maps of Europe at the period of maximum glaciation and the third Glacial

epoch respectively, and in the same book, I cannot understand; and it is inexplicable how, after such a statement, these most misleading maps should have been retained. If it means anything it means that there could be no ice-sheet shed into the North Sea at all from Western Norway, since the ice there did not extend beyond the Norwegian coast; nor, in view of this confession, can I understand such sentences as the following, approvingly quoted from Croll: "The Scottish *mer de glace* must have advanced over the bed of the North Sea to coalesce with the great ice-sheet which at the same time crept out from Norway." Dr. Geikie adopts this sentence, and then goes on to say: "This acute inference was subsequently proved to be correct by the observations of Messrs. Peach and Horne, who showed that the Orkney and Shetland Islands had been overwhelmed by the United Scoto-Scandinavian *mer de glace*" ("Great Ice Age," new edition, p. 85). Again, "during the formation of the basement clay [of Holderness] the bed of the North Sea was occupied by a great *mer de glace*, which invaded Holderness from the north-east" (id., p. 363). Again, he speaks of "the advent of the North Sea ice-sheet, which as it passed to the south-west became confluent with that broad stream of ice underneath which the Lower Boulder-clay of Yorkshire, etc., was accumulated" (id., p. 375). I will not refer again to Mr. Milne Home's acute analysis and criticism of the case for the glaciation of the Shetlands by ice coming from the north-east, as supposed to be proved by striæ coming from the north-west, but will content myself with asking how Professor Geikie, having destroyed his Norwegian ice-sheet on page 468 of his book, came to retain these extraordinary instances of contradiction with himself on pp. 85, 363, and 375. I know of no parallel to this so complete as the practice of a Scotch parson in Manchester who, having two congregations which do not agree with each other, devotes the afternoon sermon to confuting the sermon of the morning!

With the disappearance of this Norwegian ice-sheet disappear the conclusions about the Scotch ice being banked up and thrust back upon Scotland by the pressure of the great monster, and disappears also the extraordinary notion, originated, I believe, by Mr. Jack, that the so-called Tees Glacier was turned aside and had to make an elbow, and then proceeded leisurely to the south in consequence of having met with an impassable wall of ice in the North Sea. Some other cause, less fantastic and less inspired by poetry, will clearly have to be forthcoming if we are to account for the difficulties supposed to exist in either area. We still have left for consideration the Norwegian stones which occur in the maritime districts of Yorkshire, Lincolnshire, and Norfolk, and the ice-sheet which is supposed to have brought them. Let us see what is really meant by this egregious postulate. None of the Norwegian stones in question have been traced to Western Norway nor to the Dovrefelds. They have all been traced to the comparatively low-lying western border-lands of the Christiania Fjord. We are, then, to believe that

at a time when the great Norwegian range of mountains could only drive the ice on its flanks and on its shoulders down the fjords as far as the sea, and no further, the ice on the low-lying lands bordering the Christiania inlet was able to convey its load of stones right round the Naze of Norway and then across the North Sea to Yorkshire and to Norfolk! How is this to be accounted for by any sane theory? Suppose we postulate the possibility of such an ice-river as this, how was it to travel, not along a valley, where the line of least resistance would enable it to flow, and which was perfectly open to it, namely, the hollow trough running up the coast of Norway, but right across several valleys and downs in the bottom of the North Sea, that is, right across the line of least resistance. Not only must it have moved uphill and downhill, but also adopted in addition a most sinuous and extraordinary course, apparently uncontrolled by the contour of its bed, and for no reason under heaven except to supply the orthodox geologists with an opportunity to test the extreme gullibility of geological mankind in the nineteenth century.

Again, if such an ice-stream adopted such a course it must have had a tremendous push behind it, and this means that the slope of its upper surface must have been at a very considerable angle, and if so the ice must have been of portentous depth at the initial stages of the journey. This syllogistic chain of argument would be conceded, I presume, by everybody, even by the orthodox geologist. If this were the case, how were the stones ever got at by the ice at all, for the whole country must have been smothered in ice, and especially the district of Laurvig. There could be no "Nunatakkar" to supply the stones in a district so low as this if we are to have an ice-river flowing thence to England. Again, we would point out De Geers' map of the "End Morainen," or terminal moraines, of the Norwegian Mountains (see Deutsches Geol. Gesell., vol. xxxvii, Tab. xii) in this very district of Laurvig, and ask how they are to be explained at all if an ice-sheet ever came down the Christiania Fjord. Again, if this Laurvig ice-river came to England with its stones, how was it possible for the postulated ice-sheet from Westmoreland and North Lancashire to get out to sea for twenty miles, and leave there its burden of Shap Fell stones which have been dredged in numbers at that distance from our coast. Did the two monsters embrace in the middle of the North Sea and then change places, as we do in a quadrille, and dance a jig together in Holderness?

If we are to credit the account of Mr. Plant, who describes with some deliberation the finding of Norwegian boulders at Leicester, and another account which refers to a piece of rhomb-porphyr found at Cambridge, the Norwegian glacier must have extended far into England, if it came here at all. If it came so far, how was the Durham glacier to cut right across it, in order to bring its load of stones down to Essex?

And when these several glaciers all arrived, how were they to mix their various loads, here with gravel, there with sand, and there with clay, without mingling the whole of these deposits into a common hotchpotch? And how, having arrived, were they to

collect the débris from the low-lying Fen country, over which the ice would be passing fathoms deep?

And where are the terminal moraines they left? Even if, as some allege, it is not reasonable to expect such terminal moraines in the case of widespreading and all-overwhelming ice-sheets—although much is made of such supposed terminal moraines in Germany and America—where are the terminal moraines of the glaciers which are supposed to have come from Durham and from Derbyshire, and from many districts nearer still? Where are the concentric mounds? Where is there a trace of true moraine stuff—that incongruous mixture of sand, clay, gravel, and stones—to be found among these sorted beds? These beds are not heterogeneous, but are sorted in regard to contents, and sorted also in so many places in regard to locality, and spread, not as glaciers spread their burdens, which throw them down at the end of their journey in great mounds and ramparts, but distributed like a warm and beneficent blanket over the wounded and torn surface of the country, all smoothed down to levels of beautiful curvature, and as unlike the kind of thing at the mouths of Norwegian and Swiss mountain valleys as a heretic is unlike a geologist.

Lastly, to revert to an argument already used in former papers. Having come to Eastern England with their foreign stones, how did these glaciers proceed to gather from the east and from the west Chalk and Oolite and Lias fragments, and to mix them with their own burden of stones, to sort them so that they got smaller as we go farther south, and to imbed them here in gravel, there in clay, there, again, in sand, and then to redistribute them. To appeal to ice as the performer of this kind of legerdemain is to do in effect what the peasants in outlandish places still do, and to call in Robin Hood, or Mr. Labouchere, or the Devil, or anybody else that seems likely to have done otherwise impossible things. Either the possibility or the probability should be proved, or this kind of speculation should be banished from serious scientific works and memoirs, and especially from memoirs which have an official sanction.

I have tried to show that the drift beds of Eastern England in every feature known to myself, stand at the antipodes of glacial deposits, and yet the same cry will go on until this generation is extinct. It is very difficult even for Africans to break their fetishes. How much more difficult for official and other influential geologists to tell the world they have been leading them a "wild-goose chase" for fifty years. If they are not likely to listen to the Philistine from the other side of the hedge, they might, at least, listen to one prophet, whose advocacy of the Glacial theory has been otherwise extravagant. Professor Carvill Lewis, speaking of the area which has alone occupied us in these papers so far, and which on his maps he entirely excludes from his Glacial area, says: "I think that by far the larger part of England was not covered by land-ice" ("Glacial Geology of Great Britain," p. 20).

Speaking specifically of the counties of Lincolnshire, Norfolk,

Suffolk, Cambridge, Nottingham, Stafford, Warwick, etc., he says of the ice-sheet or great glacier, which is supposed to have covered them: "It would be a curious kind of glacier which could at once carry boulders from the south as well as from the north, from the west as well as from the east, mixing them all in one deposit; and which was able neither to make moraines, nor to striate rock surfaces, nor to smooth projecting crags" (id., p. 55). I quote this, not because I am in general agreement with Professor Carvill Lewis,[1] for I am not, but because he being considered an almost inspired prophet among the Neo-Glacialists, it is useful to use one of his bomb-shells against them.

Thus I have, so far as I can see, shown that it is impossible to attribute the drift deposits of the Eastern Counties of England or their contents to either ice-sheets or glaciers.

Note.—Referring to note on p. 128, March, 1897, I am reminded that hollows in the floor of the glacier could clearly only be made if the glacier remained stationary.—Edit.

V.—On the Subdivisions of the Carboniferous Series in Great Britain, and the True Position of the Beds Mapped as the Yoredale Series.[2]

By Wheelton Hind, M.D., B.Sc.Lond., F.R.C.S., F.G.S.

THE correlation of the various series of Carboniferous rocks which occur in different districts of Great Britain according to the fourfold subdivision proposed by the late Professor Phillips, has always presented great difficulties, on account of the marked changes in sequence, thickness, and lithological character which occur in districts only a few miles apart. The consideration of this difficulty was the subject of a special Sub-Committee of the International Geological Congress of 1888. The Report, however, indicated only a difference of opinion, and consisted chiefly in a series of typical sections showing the local development of Carboniferous rocks, and the schemes of classification proposed by various members of the Sub-Committee.

Of late, the officers of the Geological Survey have, to a large extent, dispensed with the term Yoredale Series, and have substituted the title *Carboniferous Limestone Series*, to denote the whole of the beds from the base of the Carboniferous to the bottom of the Millstone Grit Series in England. But this term was used by Professor Phillips to include, not only these beds, but the Millstone Grit as well ("Geol. Yorks.," pt. ii, p. 11).

Unfortunately, the term Carboniferous Limestone Series has been applied, not only to the alternating series of sandstones, limestones, and shales which are characteristic of the northern representatives of the Carboniferous Series, but has (following Professor Phillips) been used to denote the massif of the Carboniferous Limestone,

[1] By some lapse of memory I find I called Professor Carvill Lewis "Carvell Williams" in two places in my previous paper, Geol. Mag., December, 1896, p. 537.—H. H. H.

[2] Read before the Geological Society of London, December 16, 1896.

and the series of shales and sandstones above which occur in South-West Yorkshire, Lancashire, Cheshire, North Staffordshire, and Derbyshire. For, although this series of shales and sandstones appears to occupy an analogous position between the thickest bed of limestone in North-West Yorkshire or the massif of limestone in Derbyshire, and the base of the Millstone Grits, respectively, the intervening series in each case differs essentially, both lithologically and palæontologically; and in my opinion these two series, which have been so long confounded together under one common term, are totally distinct, and really form the key to the Carboniferous sequence in Great Britain.

It was unfortunate that Professor Phillips adopted the term Yoredale Series for one of the main subdivisions of the Carboniferous system, although the term was undoubtedly applicable for a local series of rocks in Wensleydale, etc., which exhibited a gradual but marked change in lithological type from those immediately below them; although there is no doubt that he was aware that the Yoredale Series [1] was only the local equivalent of the upper part of the Carboniferous Limestone, yet he insisted on correlating the beds with those which, in a different area, were undoubtedly above the massif of limestone. A single quotation [2] will suffice to show Professor Phillips' view as to the relations between the massif of limestone and his Yoredale Series: "The great limestone series which at Greenhow Hill is one mass, admits between some of its upper members partings which are thin at Grassington and Kettlewell, but towards the W., N.W., and N. augment in thickness and assume new characters." This statement is important, because it is based on the stratigraphical facts; and it is this splitting up of the upper part of the mass of limestone by beds of shale, with intercalations of sandstone further north, which gives rise to the succession of beds in Wensleydale, where they have received the name Yoredale Series; the term should, however, only be retained for these local beds, because they are typical only of a limited area.

But at the same time it follows that it is erroneous to regard the Yoredale beds as superimposed upon the Carboniferous Limestone, for they are, unmistakably, the direct equivalents of each other; and that such is the case can be shown on the strongest palæontographical and stratigraphical evidence. In the briefest possible way, I will indicate the evidence from the ground itself. One great but important objection to the use of the term Yoredale Series in any other but a local sense, is the uncertain and arbitrary line adopted as its base. In North-West Yorkshire the Great Scar Limestone was taken as the upper limit of the Carboniferous Limestone; but this limestone, although in Wensleydale somewhat massive, when traced north splits up and undergoes marked lithological changes, and becomes the Melmerby Scar Series. The Hardraw Scar Limestone,

[1] "Geol. Yorks.," pt. ii, pp. 19, 27, 32, 33, 34, and 61, pl. xxiii, Nos. 9 and 10; pl. xxiv, sections 3 and 24.

[2] p. 32.

which is the bed of limestone next above the Great Scar Limestone in Wensleydale, when traced south, gradually, by a thinning out of the intervening beds, approaches and becomes one with the massif below, so that the bottom bed of one subdivision of rocks becomes one with that next below it; and, on the other hand, in the Ingleboro area, other thin limestones, with intercalated beds of shale and sandstone thinning out south, named the Gayle and Hawes Limestones, appear between the Hardraw and Great Scar Limestones, so that it cannot be said that the line chosen was that at which the massif of limestone ceased to split up. Traced northward, the Great Scar Limestone becomes separated into distinct beds, by wedges of sandstones and shales, so that, although in Wensleydale only from eight to ten separate beds of limestone are recognized, at Alston Moor there are twelve limestones, and in Northumberland Westgarth-Foster recognized nineteen. Indeed, it may be laid down as a marked fact that, as the Carboniferous beds are traced from south to north, the organic deposits become thinner, intermittent, and separated by sedimentary beds which thicken in the same direction, and thus, by degrees, the series of Carboniferous rocks so typical of Derbyshire passes gradually into that series which obtains in Northumberland and Scotland. Calcareous deposits are essentially different in origin from those of sandstone, clays, and similar materials; for they are almost entirely derived from the remains of living organisms, and not from terrestrial denudation. Sandstones and clays, on the other hand, owe their origin to the waste material and detritus removed from land-surfaces by the action of water, chiefly in the form of rivers, and consequently denote a deposit in comparatively shallow water, laid down at no great distance from shore.

In Derbyshire and North Staffordshire the Carboniferous Limestone is practically all in one mass, containing, however, several beds of Toadstone;[1] although there may occur thin shaly partings of a few inches in thickness, a good example of which is to be seen in the quarry opposite the High Tor at Matlock, and in Combsdale, where, in the upper part of the limestone near Matlock Bath, a thin coal, with its underclay, was found.

Above the main mass of limestone there may occur, locally, a few feet of thin limestones, alternating with shales and limestones, as at Butterton Moor, North Staffordshire; but no marked beds of limestones occur in the 2000–3000 feet of shales and sandstones which overlie the limestone.

Towards the top of the limestone, and occupying the base of the escarpment at Castleton, whence it can be traced for many miles along the same horizon, is a well-marked sea-beach, with rolled shells and fragments, and an occasional quartz pebble. This was discovered by Messrs. Barnes and Holroyd, who have described the beds in a recent paper read before the Manchester Geological Society. This beach deposit came on gradually, reached a maximum, and

[1] See Arnold-Bemrose, Quart. Journ. Geol. Soc., vol. L, 1894, pp. 603–644.

then declined, as shown by the quantities of shell fragments and rolled fossils it contains. It does not form the extreme top of the limestone, but is itself overlain by a few yards of massive limestone. All the famous fossiliferous localities of Derbyshire and North Staffordshire appear to occur at horizons just immediately above or below this beach.

The next great exposure of Carboniferous Limestone north of Derbyshire is at Clitheroe, and here there is a fine sequence to be seen in the western escarpment of Pendle Hill, at the base of which the limestone is exposed. The top of the hill is formed of grit, formerly called the Yoredale Grit, now recognized by the Geological Survey as a Millstone Grit, older than the Kinderscout, or fourth grit. It is estimated that 2500 feet of shaly beds intervene between the Mountain Limestone at the base, and a series called the Pendleside Limestone of 350 feet in thickness, which occurs just below a bed of grit called the Lower Yoredale Grit. This bed is not very constant in extent, but whenever it is present it always occupies the same horizon. Mr. Tiddeman describes it in the Memoir on the Burnley Coalfield as follows: "It consists of grits and sandstones, with shales and ironstones interbedded. . . . . In appearance they (the sandstones) are often not unlike the Gannister rock of the Lower Coal-measures, though not containing the root beds of that rock; plant-remains, however, are of common occurrence in them."

The Lower Yoredale Grit is separated by about 200 feet of shales from the Upper Yoredale Grit, which caps Pendle Hill. These are known as the *Bowland Shales*, and are important as containing a molluscan fauna of Lower Coal-measure facies—*Posidonia*,[1] *Orthoceras*, and *Goniatites*. From the lists of fossils given at the end of the Burnley Memoir by Mr. Etheridge, it is conclusively shown that the majority of the species found in the Pendleside limestones is common to it and the Carboniferous Limestone below, although the beds are separated by 2500 feet of strata which do not contain a similar fauna.

The Upper Yoredale Grit of Pendle is supposed to be the equivalent of Farey's Shale Grit of Derbyshire, and with very good reason, on account of the relation which both have to the fourth and fifth, or Kinderscout grits. The upper and lower Yoredale Grits are now known as the Pendleside Grits, and these constitute the base of the Millstone Grit Series of the Geological Survey. From the top of Pendle Hill there is a regular sequence to the Coal-measures of the Burnley Coalfield, and the fourth grit forms a feature of the ground from Hoolster Hill by Mellor Walley Nab and Wiswell Moor to the Nick of Pendle. Succeeding the fourth grit are the Sabden Valley Shales, with *Posidonia*, *Goniatites*, *Orthoceras*, etc.

[1] This name is abundantly used in Lancashire and Yorkshire for at least two different forms of shells which have been studied by Mr. H. Bolton and myself. I have referred them to *Posidonia lævis* and *P. minor*, Brown, sp., and they form a part of the matter to be published in this year's portion of my Monograph on "The British Carboniferous Lamellibranchs," by the Palæontographical Society.

A similar sequence is seen from Ribchester to Chipping, across Longridge Fell, which is capped with Upper Pendle Grit. The third and fourth grits are seen in the River Ribble, north of Longridge; and below the escarpment the Lower Pendle Grit forms a feature. Below this are the Pendleside limestones, which rest on shales, and near Chipping is the Carboniferous Limestone.

At Ashnot, one and a half miles south of Newton, is the outcrop of a small patch of limestone in the shales, which, from its position, would appear to belong to the Pendleside limestones. This patch is very fossiliferous, and twenty-three typical species of Carboniferous Limestone forms are said to occur there. Mr. Tiddeman says of it (p. 19, Burnley Memoir): "It bears a stronger resemblance to the Great Scar Limestone than any other beds which I know in a like position."

No other district shows such a thickness of shales between the limestones as obtains in the Pendleside area; indeed, deposition of mud here was locally very excessive; on Pendle Hill, however, the Upper Pendle Grit is nothing like so thick as it is on Longridge Fell, where it is about 1200 feet. Traced north-westward, the beds between the Pendle Grit and the limestones rapidly diminish; and about two miles south-west of Slaidburn (the beds dipping at the average of 30° in three-quarters of a mile) we pass from Mountain Limestone to Pendle Grit, and two miles further north-west the Pendleside limestones have disappeared, and the grit is reduced to a comparatively thin bed, which forms a feature in Ramsclough.

Further north-west, in the Carnforth district, the grits and shales are still thinner, and no limestones exist in the shales between it and the grits; the Pendleside Grit, 1200 feet thick at Longridge, having entirely vanished, together with several hundred feet of shales with limestones, in about twenty miles.

Traces of the Pendleside limestones are seen in scattered localities, e.g.: near Cold Coniston, on Embsay Moor near Skipton, and at Studfold, near Draughton; but the intervening shales thin out rapidly to the east. The fauna is typically that of the Carboniferous Limestone, and almost entirely different from that which occurs in the Bowland Shales. A few miles north of Skipton the beds between the limestone and the grit thin out very rapidly, and in Niddale (vide p. 168) are reduced to a very few feet. A good section can be obtained in the streams on Burnsall Fell, between it and Hill Bolton. Here, unless there be an overlap of the grits or an unmapped fault, there are only about 300 feet of shales, and no limestones beyond the main mass, nor any features indicative of such. Only about one and a half miles north of Burnsall, which would be north-east of the line of the sequence given above, but on the other side of the Craven fault, a distinct limestone, but only one, is seen to occur in the shales between the grit and the limestone. Tracing this bed northwards, it forms a feature north of the village of Hebden, and it is supposed by the Officers of the Survey to split into two beds north of Grassington, which become amalgamated with the main mass of limestone still further north. Phillips gives at p. 30,

"Geol. Yorks.," ii, a measured section of the beds on the west flank of Great Whernside, as follows :—

		ft.	in.
	Bearing grit	100	0
277 ft. 6 in. Upper Limestone Group.	Plate	84	0
	Sharp, hard limestone	6	0
	Parting		
	Light-coloured limestone	9	0
	Parting		
	Light-coloured limestone	36	0
	Parting		
	Light-coloured limestone	60	0
	Plate	4	6
	Dark-coloured limestone	76	0
	Limestones and some gritstones and plate ...	210	0

It will be noted that the figures given of his Upper Limestone Group total up to 277 ft. 6 in., so that the partings are so thin as to be practically neglected, and the only bed of shale of any measurable size, 4 ft. 6 in., which might have served as a base-line, is not so used, but apparently hypothetical lines are chosen for the subdivision.

Tracing the beds to Starbottom and Bishopdale, the shale beds gradually thicken to the north-west, and beds of sandstones become intercalated with them; and this series of beds, which measures 277 ft. 6 in. at Great Whernside, is increased to 510 feet at Starbottom.

If the Carboniferous succession be examined on the western side of the Pennine anticlinal, the first indication of any limestone between the main mass and the Millstone Grit is to be seen in the Outside Pastures east of Lancliffe Scar, between that locality and Malham Tarn. This patch of ground is much faulted, but a bed of dark limestone, with a low dip, is seen in the shales of the brooks, and it forms a feature in Black Hill, which is capped with Millstone Grit. It is quite possible that this bed may be the equivalent of the Pendleside limestones, but if so the shales are much diminished, especially between it and the limestone mass below.

In the side of the hill below Black Hill is a patch of ground where a small stream comes out at the junction of the limestone and the shale below it, called Clattering Sykes. This stream washes out thousands of encrinite rings and a few other fossils, as *Orthis Michelini*, *Spirifer*, *Productus*, etc., which are left on the ground. These are doubtless derived from the shale bed, which holds up the water which has sunk through the thin bed of limestone, and show the continuity of the fauna of the mass of limestone during the deposition of the shales.

Three miles north of Black Hill is Penyghent, with 500 feet of strata between the main mass of limestone and the Millstone Grit which caps it, and in the 500 feet are no less than four beds of limestone. In Ingleborough this series measures 900 feet, and on Whernside 1000 feet, and the Great Scar Limestone is only about 600 feet thick, so that here the whole series is no thicker than the Mountain Limestone of Derbyshire.

Immediately north of the districts mentioned above is Wensleydale,

where the beds, with from five to eight thick beds of limestone, to which the name Yoredale Series was given, are typically developed; and they extend to about 1500 feet in thickness, but the Great Scar Limestone below this series is only estimated to be 600 feet. In the south of Yorkshire the massif of limestone itself is probably about 2000 feet thick, but its base has nowhere been seen. This diminution in deposit is not in any way due to denudation, for no evidences of unconformity are to be observed. The Carboniferous Series as developed in Wensleydale can be fairly well traced northwards into Swaledale and Teesdale, but still north of this there is a tendency to subdivision and variation, and the Great Scar Limestone becomes the Melmerby Scar Series.

In the neighbourhood of Ash Fell and Roman Fell an interesting series of sandstones with plant-remains, limestones of tolerable thickness, conglomerates, and shales, is developed below the Great Scar Limestone, the most southern trace of the Calciferous Sandstone Series so much more fully developed in Northumberland and Scotland, and which would appear to be stratigraphically below the Carboniferous Limestone mass of Central England.

In certain districts of England the Carboniferous Limestone is immediately overlain by the Millstone Grit (Coalbrookdale and Clee Hill). This is generally said to be the case in Flintshire also, if the beds classed as Millstone Grit really belong to that formation, which I doubt on palæontological grounds. In the Forest of Dean the intervening beds are estimated at 116 feet (Wethered). At Clifton about 300 feet of shales, sandstones, and limestones occur between the grits and the main mass of limestone, and here the limestone fauna is largely present. In South Wales the Carboniferous Limestone is succeeded by the Gower Series of about 1600 feet of black shales and sandstones. At Tenby they are only represented by a few feet, with beds of limestone, and contain Goniatites. Mr. H. B. Woodward ("Geol. Eng. and Wales," p. 167) says they remind one of the Black Limestones of North Devon that occur at the base of the Culm-measures.

In Europe and North America the intermediate beds between the calcareous beds of the Carboniferous period and the Coal-measures seem to be to a large extent wanting, and the misfortune is, that an attempt has been universally made to correlate these beds with those of a series in Great Britain which have been chosen as a type, and to identify certain well-marked beds with others which have a purely local character. A fuller and more accurate knowledge of Carboniferous palæontology will, however, in time establish a better basis for comparison.

In the Carboniferous Series of North Wales, Mr. Morton has shown (Proc. Liverpool Geol. Soc. 1881–2) that the following series obtains:—

Cefn y Fedw Sandstone.
Upper Black Limestone.
Arenaceous Limestone.
Upper Grey Limestone.
Middle White Limestone.
Lower Brown Limestone.

The Geological Survey has considered the uppermost member of this series to be the equivalent of the Millstone Grit, because it passes into a pebbly sandstone further south. Mr. Morton has shown that at Pentre Halkin the white shale interstratified with the chert beds contains *Chonetes Laguessiana* and *Productus longispinus*. Several other typical Lower Carboniferous species are stated to be found in these so-called Millstone Grit beds, on the authority of the Survey; but I am bound to admit that *Aviculopecten papyraceus* and *Posidonomya Gibsoni*, whatever may be meant by this last name, are credited the former to one and the latter to two localities, undetermined Goniatites also occurring with the former.

Mr. Morton observes (Trans. Manch. Geol. Soc., vol. xvii, p. 81, 1883): "As the fossils found in the Cefn y Fedw sandstone are all common limestone species that occur in Denbighshire and Flintshire, it seems possible that the formation is an arenaceous continuation of the Carboniferous Limestone."

At Saith-aelwyd the chert beds are immediately above the Black Limestone. The section of the chert beds at Afongoch[1] gives—

	feet.
Banded chert	6–8
Chert full of encrinites	3
Chert and limestone—sometimes all crystalline limestone, sometimes containing blocks of chert with a heart of limestone, or vice versâ	1
Banded chert	8

It is definitely demonstrated that the chert has either replaced limestone or was deposited at the same time. It is very questionable, therefore, whether strictly any equivalents of the Millstone Grit occur in North Wales. It must be remembered also that, although sandstones of various degrees of coarseness are known in the Lower Carboniferous Series, chert beds are conspicuously absent in the Millstone Grit Series of Lancashire and Derbyshire.

I have been impressed with the occurrence of beds of stratified chert in the Upper Yoredale Series of Wensleydale, and the fact that similar beds occur in the upper portion of the Carboniferous Limestone of Derbyshire; and, although I would hesitate to say that this occurrence of beds of similar chemical characters in the upper beds of the two series is any positive evidence as to these beds being either the chronological or homotaxial equivalents of each other, I give the fact for what it is worth as indicating one other feature which the massif of Carboniferous Limestone and the Yoredale beds of North Yorkshire have in common.

I have written elsewhere (Geological Magazine, Dec. IV, Vol. III, p. 259):—"The Carboniferous series of Northumberland and Scotland are essentially characteristic of those [beds] deposited near a shore which occasionally became land, and occasionally was depressed deep enough to allow limestones to accumulate on its floor. The land which was the source of the muds and sandstones was probably of no great elevation, and consequently river-action

[1] Mem. Geol. Surv., "Geol. Flint, Mold, and Ruthin," p. 48.

was not strong enough to carry the sediments far out to sea." The shore-line was by no means constant in position, but advanced south and retreated back again according to the varying rates of sedimentation and depression.

No real break is, therefore, to be found in tracing the variations in the Carboniferous beds of England from south to north, and the great change of character is certainly due to conditions of deposit. I think, in spite of recent utterances, that the great mass of the Lake District could never have been covered by the Carboniferous sea, but remained an island or peninsula during that period. I would insist that deposits of sands and shales are essentially the débris of land, and are always shallow-water deposits; and in the case of Carboniferous sandstones and shales, the presence of plants in too large numbers to be attributed to drifting by wave-power, is conclusive evidence that such was the case. Marine erosion practically only takes place for a short distance beyond low-water mark, and an examination of conglomerates points to the locality whence the fragments were derived. The fragments of Silurian rocks which are found in the Basement Conglomerate bed are nearly all subangular, and have therefore not been rolled much or carried far; but apart from evidence it is a necessary conclusion that encroachment of the sea must have been gradual, and therefore there must have been an advancing or retreating shore for some period of time or other after the earth-movements, which threw the older rocks at such an angle that the basement beds of the new (Carboniferous) deposit were laid down on the fractured and upturned edges of the older beds.

In the South Midlands depression and sedimentation seem to have gone on at a fairly uniform rate for a long period of time, interrupted only by conditions which have left their mark in their shaly partings, rarely exceeding more than a few inches in thickness. In the North and nearer the shore-line, sedimentation was often so far in excess of depression that the terrestrial conditions necessary for the growth of the coal flora obtained. But there came a time even in the southern area when the deposit of limestone became less constant, and instead of the massive deposit the beds are thinner and assume a more fissile character, these being succeeded by shales containing thin limestones, which in turn are covered in by thick beds of black shale, with occasionally beds of fine, hard, gannister-like sandstone, all of which are, however, peculiar to the Derbyshire and South Yorkshire area. These beds, in common with those of Wensleydale, have received the name of Yoredale Beds, but, unlike them, contain no limestones, except a few irregular, thin bands at the extreme base. If I am correct in my contention that the Yoredale Series of Wensleydale are simply the equivalents of the upper beds of the Carboniferous Limestone (and several authors have seemed to acquiesce in this view) and do not overlie it, then the term Yoredale can no longer be retained for the mass of strata which intervenes between the Millstone Grit and the Carboniferous Limestone in South Yorkshire, Derbyshire, and North Staffordshire.

It would appear that this series is practically absent in the north of Yorkshire, or only represented by a few hundred feet of shale immediately below the Millstone Grit. The officers of the Geological Survey have of late years relegated all the beds to the base of Farey's Grit, called also the Shale, Yoredale, or Pendle Grits, to the Millstone Grit Series, and they further raise the question ("The Geol. of the Carb. Limest. Yoredale Rocks and Millstone Grit of N. Derbyshire," p. 7: Mem. Geol. Surv.) as to whether the quartz gannister-like sandstones of North Staffordshire should not also be grouped with this division. They argue that the question depends on the condition under which these beds were deposited. If these conditions were marine, then the beds are to remain Yoredale; if estuarine, to be included in the Millstone Grit. In the absence of fossils they are retained provisionally as Yoredale beds. Now these quartzose rocks do contain plant-remains in certain localities—Gun Hill, Leek Waste, and Congleton Edge—and may therefore be said to be non-marine, and consequently the Yoredale sandstones of North Staffordshire fulfil the conditions laid down by the Survey for classification as Millstone Grit Series. But the matter is really not quite so simple as all this, for occasional marine bands are to be seen in the shales with bullions which separate some of the gannister-like beds containing a typical marine fauna. Indeed, there are two distinct marine faunas present in the shales, which do not intermingle,—one typical of the deposit and the beds above,—the other consisting of species common to it and the beds below, and only very local in its occurrence.

That which I call the typical fauna of the deposit, and which comes in at this horizon for the first time, is itself a very important one, and differs very markedly and in every particular from the fauna of the Carboniferous Limestone and its equivalents in Wensleydale and Scotland, in which districts no trace of the other group of organisms has been found.

This series of black shales with sandstones are exposed in the bottoms of the valleys in South-West Yorkshire, at Mossley, Saddleworth, Marsden, and further north at Todmorden, Hebden Bridge, Sabden, and Flasby, and the escarpment of Pendle Hill, in Lancashire at Longridge Fell, and are overlain by the Millstone Grits. I have alluded to the latter sections above, and will merely repeat that this series rapidly thins out to the north, until the mining section at Greenhow Hill shows only 12 feet of limestone and plate between the lowest Millstone Grit and the main mass of limestone. The late J. W. Davis ("West Yorks.," p. 76) says:—"In Upper Wharfedale and Nidderdale there are about 20 feet of shale and dark-coloured limestone between the Lower Limestone and the Kinderscout Grit. The course of the River Nidd above Lofthouse runs in the Lower Scar Limestone. This is seen covered by 4 ft. 6 in. of shale with thin clayey limestones in its upper part; then follow 6 feet of blue limestone, 14 inches of shale, and 5 feet of black, compact, laminated limestone. Above the laminated limestone the lower beds of the Millstone Grit crop out."

As far as I can see, the only stratigraphical difficulty which presents itself is the occurrence of the Pendleside limestones at such a distance from the main mass as 2500 feet. But it appears to be very local in its development, and it is certainly not to be traced to any distance except perhaps to the north-east, while the great mass of shale itself thins out very rapidly in the same direction. The Pendle Hill and Longridge sections are extremely valuable, for they both exhibit a complete sequence from Coal-measures to the Carboniferous Limestone. It must also be remembered that the whole of the Millstone Grit Series is really a very local deposit, only appearing in force in South Yorkshire, in Lancashire and Derbyshire, rapidly thinning out both to the north and south.

(*To be concluded in our next Number.*)

VI.—Foreign Boulders in the Chalk.

By Edward A. Martin, F.G.S.

THE recent find by Mr. Stebbing, F.G.S., of two large boulders of granite in the Middle Chalk of Betchworth (Surrey), has called to mind the various discoveries of boulders which had previously been made in the same marine formation by other observers.

I am not aware whether the discovery of the so-called Purley syenite boulder (from the Haling Pit, South Croydon) has ever been considered in connection with that remarkable boulder which was dredged up by the "Challenger" off Halifax. This, too, was of syenite, and weighed no less than 5 cwt., and was brought up from a bottom of grey mud containing many foraminifera. Whatever could be said on behalf of some of the boulders from the Chalk having been conveyed by floating seaweed, or the roots of trees, the probabilities are all in favour of this block having been transported by ice, in a sea where so many icebergs are found at certain seasons of the year. And this without the necessity of inventing a special Glacial Epoch. We may argue from the one to the other, and without any very great stretch of imagination see the work of an occasional iceberg in the occasional occurrence of a boulder of foreign material in the Chalk.

The great Purley or Haling boulder, the boulder from the Gayton Pit, Norfolk, and others, are all thoroughly rounded. From this reasonably follows that they must have suffered much before being imbedded in the iceberg, perhaps on that shore which existed around those portions, comparatively small, of modern Europe, which even then were raised high and dry above the Chalk sea. The Betchworth boulders are not nearly so rounded. Added to this, fragments of *Spondylus* and *Serpulæ* are still attached thereto. Icebergs take a long time to melt, and I think there is just as much likelihood of *Serpulæ* becoming attached to a submerged block of granite as barnacles to a ship's bottom. At the same time the presence of *Serpulæ* seems to show that there could not have been a "Glacial epoch."

Whence flowed the currents which bore the icebergs? Dr. W. F. Hume said from the south. So far as I know, the Dartmoor granite does not vary much. At any rate, I can speak for the great resemblance of the Betchworth granite boulder "B" to the granite of Hey Tor and that of Lustleigh Cleave, and thereabouts. The large crystals specially bear a great resemblance. Professor Bonney gave a tentative opinion as to their Scandinavian origin, but apparently with no great degree of positiveness. I suggest, therefore, that the conveying current may have been from the south-west. The Dartmoor Tors have undoubtedly suffered much denudation. Perhaps they extended above the level of the Cretaceous sea, and gave rise to an Alpine glacier even in the midst of an equable climate.

As to the decomposition which Mr. Stebbing's boulders have undergone, I think we may assume that the change has taken place since their interment in the Chalk. I understand Mr. Stebbing did not actually see them excavated, otherwise it would have been interesting to have known whether there were smaller fragments or any sand around them, as in the case of the Purley or Haling boulder. I saw some blocks of chalk and a somewhat similar granite, which had been used together as building materials, in the foundations of the old Blackfriars Monastery. These were excavated about 1886, near the western end of Knightrider Street, to make room for some new post-office buildings which were then being erected. The chalk was comparatively intact. The granite was in just such a condition of disintegration as was exhibited by the two Betchworth boulders.

REVIEWS.

I.—Microscopic and Systematic Study of Madreporarian Types of Corals. Philosophical Transactions of the Royal Society of London, vol. clxxxvii (1896), pp. 83–345. By Miss Maria Ogilvie, D.Sc.[1]

THE criticism which a work like this calls for may be taken as an index of its importance. No one who has worked at the corals will deny that Miss Ogilvie has the merit of advancing our knowledge by a considerable amount of solid work; we warmly congratulate the gifted authoress upon it. But the critic's task is to criticize, and the pleasanter congratulations must be hurried over in order that we may endeavour to estimate the value of the work done. It must be understood that I set this task before myself as a reviewer in no arrogant spirit, as claiming the superiority of a judge. A criticism is no more infallible than the work criticized. A review at its best is but the calm and impartial opinion of a fellow-worker in the same field. As no two workers approach a subject from exactly the same point of view, no two having worked with the same material, or treated what material they have had in exactly the same way, everything is to be gained by free criticism of one another.

[1] Mrs. John Gordon.

While not here primarily concerned with the work as a literary production, it is impossible to avoid expressing a regret at its really great and needless length. Further, one notes a looseness in the use of certain words which is harmless unless the word itself happens to be important. The authoress's use of the word "homologous" is so ambiguous, indeed, that some of her sentences, even when their real meaning as gathered from the context is correct, do her injustice. For instance, on p. 158, "a single branch of the *Mussa corallum* is the homologue of the whole massive corallum of a *Heliastræa* or *Goniastræa*"; and again, "a single tooth of a *Mussa* septum is the exact homologue of the complete septum of *Galaxea*."

Coming now to the scientific portion of the work, in a short review it is only possible to refer to the leading points. Von Koch, Fowler, and Bourne had already established that the coral skeleton was secreted by the ectoderm; Miss Ogilvie finds that this takes place by the actual calcification and death of successive layers of ectoderm cells, the supply of these cells being maintained by cell division. Miss Ogilvie does not seem quite to appreciate the importance of this latter statement; she merely asserts it. The death of the ectodermal cells and their utilization as hard units in the formation of epidermal structures is well known in the animal kingdom, but as to the method of replenishing these cells much has still to be learned; hence, while not doubting Miss Ogilvie's observations, zoologists will be generally disappointed that she dismisses this very important part of the observation without any figure, and in less than a single line.

The septa being secreted in folds of the ectoderm are composed of two apposed layers of skeletal matter. These septa not only rise in height, but mutually support one another in different ways: for instance, local thickenings of adjoining septa, or granules from their faces, may meet and fuse. Here is Miss Ogilvie's second solid contribution, continuing and amplifying the work of Pratz. She finds that these septa with their outgrowths are not only built up of discarded calcified cells, but that these cells are grouped into units of structure of different orders and degrees of importance. Before discussing these structural elements and her application of them, I must call attention in passing to a doubtful use of words. In describing these septa and the outgrowths from their lateral surfaces, Miss Ogilvie continually speaks of invaginations of the wall being filled up by skeletal matter. On p. 125 we find: "Is the ectoderm smooth, so is the skeletal lamella. Is the ectoderm wrinkled, pitted, grooved," etc. See also pp. 137, 138. On p. 180: "New invaginations . . . . give rise to new centres of calcification." And again, "invagination" is sometimes most confusingly used where the object referred to is the skeletal part contained in it. Is not this a begging of the question and thus misleading? Are septa and their surface-granules and synapticulæ formed by active infoldings of the body-wall which are secondarily filled up by coral substance? There seems to me some confusion of thought in the use of this

terminology. It is true that when any portion of the basal body-wall resting upon a single layer of the secreted skeleton leaves that layer, it has undoubtedly been pulled off by the growth of the skeleton raising up the whole animal. In this case the skin typically proceeds to secrete another layer as soon as it has come to rest, these secondary basal layers forming "dissepiments" or "tabulæ," according to their position in the calyx. It appears as if this active withdrawal of the body from its flat skeletal floor has led Miss Ogilvie to attribute the formation of septa also to active infoldings of the wall. Is there any evidence for this? My own impression was, that the active secretion of solid matter by the aboral ectoderm was prior to the formation of septa, and thus sufficient of itself to account for the septa. The more active secretion of the ectoderm in the bases of the intermesenterial chambers would cause the passive pushing in of the aboral wall by radially arranged deposits. Wherever, in fact, for any cause, the secreting activities are increased the wall is passively pushed in by the hard matter deposited. The very disordered order of the centres of calcification revealed to us by Miss Ogilvie's sections is, it seems to me, more in keeping with this suggestion than with what appears to be Miss Ogilvie's own view of the matter. But, however clear and correct Miss Ogilvie's own views on this subject may be, she certainly leaves the reader in doubt as to whether the invaginations of the body-wall are active or passive. This is not by any means a trifling point, for morphology is a vain pursuit without physiology.

The discarded calcified cells, then, are the bricks for the building up of the various parts of the skeleton, and while they are laid fairly regularly in layers ("growth lamellæ") over one another, these layers never seem to start quite regularly except when the deposit is single, e.g. for the formation of a dissepiment. On the other hand, whenever these bricks are going to bulge up the skin, which secretes them, for the formation of a septum, they seem to start, not along continuous lines, but from single points round which they radiate. These dark points are "the centres of calcification," while the cells with their crystalline contents form radiating bundles of fibres called "fascicles." Sections of septa show fairly regular groupings of the dark centres with their fascicles. A trabecula, according to Miss Ogilvie, is formed by a continuous series—single or multiple—of such dark centres rising from the base towards the upper edge of the septum, straight up or bending towards its inner or outer edge, or sometimes even horizontally arranged: the distal ends of these trabeculæ run out into teeth or septal spines, and so on: for the many interesting details the reader must consult the original work.

Miss Ogilvie, whose merit it is to have patiently examined for these details of fine structure a long series of types, recent and fossil, is able to announce that this method of building up the skeleton is the same to-day as it was in the earliest known fossil; growth lamellæ, fascicles, and trabeculæ are fixed elements

in coral skeletons, and are as much units of structure, only of a higher order, as are the ultimate calcified cells out of which they are built. The importance of this to the coral worker, inasmuch as it helps to clear the view and correspondingly heightens his confidence in attacking the morphological puzzles that beset him at every turn, is certainly great. But the new weapon which Miss Ogilvie has given us she proceeds to use herself, and, while heartily acknowledging the debt, I am constrained to say that I cannot agree with the use she makes of it. I write entirely for myself, and Miss Ogilvie may take this comfort to herself, that she has the majority with her, and sins, if it is sinning, in good company. On the other hand, she seems to me to have carried this tendency to an extreme, which it is to be hoped will inaugurate a wholesome reaction.

In 1834, Ehrenberg, recognizing the coral stock as the product of a budding process, made the method of budding the factor of prime taxonomic importance. In this he was followed by Dana in 1848, and by Milne-Edwards and Haime. Methods of budding were, however, obviously inapplicable for the classification of solitary corals; hence characters relating to the structure of the calyx had to be sought. The researches of Heider, Von Koch, Fowler, and Bourne have been supplying us with the main differences in the method of building up the coral skeleton. These anatomical studies have now been, as it were, capped by Miss Ogilvie's researches into the minute histology of the skeletal parts.

Having come to the conclusion that Milne-Edwards and Haime had "practically exhausted the subject from a macroscopic aspect fifty years ago," and further that "habits of the corallum" should not "outweigh considerations of fine structure," Miss Ogilvie started "hoping to find some fairly embracive [sic] series of different structural types if such existed in Madreporaria. The result has proved that structural types do exist and form a surer method of determining Madreporarian genera and groups than any other method known to me." As we have seen, the differences of fine structure, on which Miss Ogilvie relies for the classification of the corals, relate to the various groupings of the fascicles with their dark centres and consequently of the trabeculæ.

It is a well-known human failing to treat (perhaps not theoretically, but practically) each new set of differences, especially if they are really important, as if they alone were of any value. There has been a distinct tendency to classify corals according to the anatomy of the calyx, the older, purely macroscopic characters being put very much in the background. And now we have Miss Ogilvie practically proposing to classify corals according to the histology of their skeletons.

I am, of course, far from objecting to the utilization of skeletal structure or texture in arranging the corals. But hastily to assign to these the first place in face of our ignorance of the interdependence of the vital processes themselves, seems to me somewhat unphilosophical. It is quite possible that we shall ultimately find

that the structure of the calicle wall, or even that the minute structure of the septa, will supply us with the means of discovering the evolutionary history of the stony corals. But, in the meantime, I frankly confess that I do not personally regard these sets of differences now under discussion as of the very first importance, inasmuch as, in many cases, they can be shown to be secondary, and dependent upon the life of the secreting organism. I accordingly, perhaps perversely, agree with the older observers who ranked first and foremost features most intimately associated with habit of life. It seems to me that differences in structure and texture gain their real importance only as they reveal to us differences of habit. It is thus not a question of rejection of characters, but merely of selection for first, second, and third place. I can perhaps make my meaning clearer by coming once more to close quarters with the work we are discussing. On page 329 Miss Ogilvie says: —"My own opinion is that the most intelligent way of studying corals is not to approach them by means of any fixed system of subdivisions, but by some such way as I have tried in drawing up the phylogenetic scheme below, where the worker first studies the anatomy of the soft parts and the structure (microscopic) of the skeleton in one common type [sic] of each of the living families of corals. According to my classification there are ten of these." This is very good as far as it goes, but the fundamental error in it is that it supplies a miserably insufficient basis on which to build up a natural system. A few, i.e. ten, of the more striking variations in coral structure is all that can possibly be obtained by this method. Miss Ogilvie would be the last to admit that the countless transition forms revealing the lines along which these variations have travelled, are to be left entirely to the fertile brain of the individual, or even to be gained by a cursory oversight of a few intermediate specimens gathered at random here and there. Further, Miss Ogilvie's chief types are in reality selected in the first instance at random. What can be said for such a principle of selection as the following? (p. 326): "Besides, I hold that, as the Poritidæ are reef-builders, on the grandest scale known in the Madreporaria, it is the reef-type which should be taken as the standard for the family, and not the small astræiform colonies formed by some species." And why is *Turbinaria mesenterina* chosen as specially typical of the genus: may it not be an extreme form?

But this is not peculiarly Miss Ogilvie's method. The tendency to exaggerate the morphological value of the one or two isolated types studied is almost universal. For instance, the detailed study of the development of a single mite has recently been made the basis for a lengthy discussion on the phylogeny of the whole class Arachnida.

Indispensable as is a thorough knowledge of types of calicle structure, I repeat that it is not enough to build a phylogeny upon. These types are not fixtures; they are only stages in the evolutionary processes, and the only philosophical method of dealing with them

is to endeavour to find out the lines along which they have travelled. This can only be done by careful comparison of the available generic and specific variations presented by each. I venture to think that the importance of not a few structural features of the calyx would be found to melt away before such a process. By way of an example I may note that Milne-Edwards and Haime united the genus *Montipora* with *Porites* on account of the trabecular character of the septa of the former. A more extended survey of the genus *Montipora* would have shown that the variation in the structure of the cœnenchyma and of the anatomy of the calyx is very great; and that the trabeculæ which are so pronounced in a large number of Montiporan types are purely secondary, and that every stage in their development can be traced from the simpler types which ally the genus with the Madreporidae. Again, Miss Ogilvie speaks of the "importance" of certain structural features in the columella of *Turbinaria*. Her type-method has led her to assume that what she finds in *T. mesenterina* was necessarily of importance. I have taken the trouble to look at about a dozen different types of *Turbinaria*, and did not find a single other type showing Miss Ogilvie's important feature. The columella in *Turbinaria* is astonishingly variable. Again, in dealing with the same coral, Miss Ogilvie assumes that the wall structures are formed nearly contemporaneously with the septa, and thus the interseptal loculi are typically cut off round the rim from the canals of the cœnosarc. A more extended survey of Turbinarian types would have shown Miss Ogilvie that the interseptal loculi very frequently run into the surface cœnosarcal canals. Or, again, Miss Ogilvie compares a calyx of *Madrepora* and a calyx of *Turbinaria*, the calicles taken at random. Anyone who has spent any time studying and comparing these genera must be convinced that their highly specialized method of budding has profoundly modified the daughter calicles. The axial calyx in *Madrepora* and the skeleton of the single parent polyp of the Turbinarian stock before the ring of daughter polyps have begun to form the cup, are the only ones that could be relied upon for structural comparison with other coral types.

But if this doubt prevails as to the supreme taxonomic importance of the grosser features of calicle structure, what are we to say of the finer texture of the skeletal parts upon which Miss Ogilvie would found a natural classification. A mere formative tissue, following, as it must do, every form variation, however insignificant, is a shifting sand upon which no one should try to build up a phylogeny. Miss Ogilvie could only establish her case by showing that the different units of skeletal structure, fascicles, and trabeculæ had some clearly recognizable morphological significance as such and were not merely component parts, produced wherever required, more or fewer as the case might be, of no more significance than are the individual fibrils in a muscle or nerve fibres in a nerve strand. A certain ordered complexity of structure is not enough: the muscle fibril is an organized body of marvellous complexity, yet it has no morphological significance whatever in settling the

homologies of the muscles themselves. How entirely Miss Ogilvie has missed this may be seen from her "homologies" already quoted, and from the difference, asserted and reasserted, between synapticulæ and pseudo-synapticulæ. If the septa are near enough for two granules to meet and fuse, it is a pseudo-synapticula; it is a true synapticula only if the space is too wide and a new "fascicle" or bundle of calcified cells has to be formed so as to unite them. A figure is given showing two septa converging towards and meeting in the centre of the calyx; where the space between these septa to be bridged is wide the junctions are called synapticulæ, but towards the centre they are called pseudo-synapticulæ, the sole difference being that more formative material was wanted to bridge the wider than the narrower interval! We need, I think, hardly wonder if classification on such lines as these results in the establishment of "mixed types."

As an example of Miss Ogilvie's method, let us take her conclusion that the cœnenchyma of the Madreporidæ is not a true cœnenchyma, but a "mural" structure homologous with the root-processes of *Rhizotrochus*. Miss Ogilvie distinctly tells us, p. 268, that but for the "*identity, in groups apparently wide apart, of the microscopic structure of the septa and of the wall,* as well as the similarity in the internal plan of the calyx," this "homology" would never have occurred to her (the italics in the quotation are not mine). In order to work this out she has to deny that the Madreporidæ possess a true "edge-zone" (or Rand-platte of Heider), because when the mural and septal structures grow up contemporaneously the interseptal loculi are not continued over the wall into the extrathecal canal system! Such a statement as this would have forced us to conclude that she did not quite grasp the relations between the polyp and the skeleton, if her own figures had not shown the contrary. She entirely rejects Bourne's suggestion that the absence of the remains of mesenteries outside the calyx may be due to secondary degeneration.

The whole argument seems to me singularly unfortunate. It looks likes a needless turning away from the obvious teaching of her own figure, which shows the relation of the flesh to the skeleton in *Turbinaria*. Again, it is not true that the interseptal loculi of *Turbinaria* are typically shut off from the cœnosarcal canals round the lip of the calyx; extraordinary variation occurs. Lastly, it seems to me that the teaching of the epitheca is in direct opposition to her contention. At least two of the Madreporidæ, viz. *Astræopora* and *Montipora*, can be shown to start colony-building in a small saucer-shaped epitheca. There is a larger central polyp, which is the parent polyp of the colony, and an irregular ring of smaller daughters between the central polyp and the epitheca. Nothing but the severest demonstration would persuade me that that epithecal saucer is not the epitheca of the parent polyp, and that the flesh which stretches from the edge of the parent calyx to the epitheca is not an edge-zone, no matter how complicated the skeletal framework underneath it may have become. The

cœnenchyma which surrounds the parent calyx, and in which the young calices are opening, can be quite naturally accounted for by the secondary alteration of the primitive costæ, an alteration which would necessarily involve the degeneration of the extracalicular portions of the mesenteries. Further, *Madrepora* and *Turbinaria* are best explained by deducing them from such a parent polyp by the different relative growths of the parent and its ring of daughters, the primitive epithecal saucer being left behind. In *Madrepora* the parent shoots up, and somewhat undeveloped daughters spring in tiers from its sides. In *Turbinaria* the ring of daughters grows up and forms a cup. The detailed evidence for this line of argument to be deduced from *Montipora* will be the subject of a paper which has already been some months in preparation. None of Miss Ogilvie's arguments on this subject seem to me to have any weight as against this much simpler and more natural reading of the facts.

The value of the rest of Miss Ogilvie's system must be left to the judgment of those who have worked over the different points. There can be no doubt that long concentration upon, and wide survey of, coral form-variations have given her an insight which her, to my mind, mistaken method cannot wholly vitiate. Her circular arrangements of the families and genera is an excellent device, though one is staggered at the boldness with which she has filled in the details. We may, however, accept these as her suggested provisional arrangement, which further and profounder study of coral variations, with the special object of tracing the lines along which these variations have travelled, will gradually emend, until our common goal has been reached, viz. a natural system of the Madreporaria.

In the foregoing pages, I have freely criticized Miss Ogilvie's conclusions from my own point of view. But whatever difference of opinion there may be between us as to what conclusions may, and what may not, be justly drawn from the new facts with which she has enriched us, there can be no difference of opinion as to the facts themselves. I am convinced that these will prove invaluable to all students of coral morphology. HENRY M. BERNARD.

STREATHAM, S.W.

II—ON THE GASES ENCLOSED IN CRYSTALLINE ROCKS AND MINERALS. By W. A. TILDEN, D.Sc., F.R.S. Proc. Royal Soc., vol. lx (1897), p. 453.

AFTER referring to the fact that many crystallized minerals contain gas enclosed in cavities in which drops of liquid are also frequently visible, the author alludes to his researches on Peterhead granite, which show that when heated in a vacuum, it gives off several times its volume of gas, consisting, to the extent of three-fourths of its bulk, of hydrogen. Since these observations were made Mr. A. W. Wright has obtained hydrogen from a certain "trap" rock in America, and Professor Dewar and

Mr. Ansdell have found hydrogen in both gneiss and felspar. Professor Tilden has now examined granophyre from Skye, gabbro from the Lizard, pyroxene gneiss from Ceylon, gneiss from Seringapatam, and basalt from Antrim, and he finds all to yield permanent gas when the rocks are heated in a vacuum. This gas varies in amount from a volume about equal to that of the rock to about eighteen times that volume. It usually consists of hydrogen in much larger proportion than that found by the observers just quoted, together with carbon dioxide and smaller quantities of carbon monoxide and hydrocarbons.

Referring to the presence of marsh gas (CH_4) in the several rocks examined, Professor Tilden observes that the occurrence of this gas, and the production of large quantities of hydrocarbonous gases, as well as liquid petroleum, in many parts of the earth's surface, tend to support the view, which is apparently gaining ground, that in the interior of the earth's crust there are large masses, not only of metal, but of compounds of metals, such as iron and manganese, with carbon. Assuming the existence of such material, it is easy to conceive how, by the action of water at an elevated temperature, it may give rise to metallic oxides and mixtures of hydrogen with paraffinoid and other hydrocarbons. This view was put forward some years ago by Mendelejeff, and has been supported in the article, previously noted, by Moissan.

III.—Chemical Geology: Étude des Carbures Métalliques. By M. Henri Moissan. Proc. Royal Soc., vol. lx (1896), p. 156.

THE author gives a summary of his remarkable researches on the formation of metallic carbides by the aid of the electric furnace, and on the compounds produced by the action of water on these bodies.

The carbides of calcium, strontium, barium, and lithium, when acted upon by water at ordinary temperatures, give rise to pure acetylene. Carbide of aluminium gives rise to pure marsh gas. Carbides of cerium, lanthanum, yttrium, and thorium yield a mixture of acetylene and marsh gas. Carbide of manganese yields a mixture of marsh gas and hydrogen. Carbide of uranium gives rise to marsh gas, hydrogen, ethylene, and also to solid and liquid hydrocarbons.

The author calls attention to the bearing of the facts on certain geological problems. Exhalations of marsh gas may, in certain cases, be due to the action of water on carbide of aluminium. Petroleum and allied compounds, which are in some instances undoubtedly due to the decomposition of organic matter, may in others be formed by the action of water on certain metallic carbides at a high temperature, as suggested by Mendelejeff.

It is probable that in the early periods of the earth's history nearly the whole of the carbon was combined with the metals. The intervention of water would give rise to hydrocarbons, and these by oxidation would produce carbonic acid. The assumption

of the existence of metallic carbides in the interior of the earth will explain the formation of hydrocarbons and other phenomena observed in connection with volcanic eruptions.

J. J. H. T.

IV. — Geology of Africa.

THE Geology of Africa in relation to its mineral wealth is the title of a paper read before the Federated Institution of Mining Engineers in 1896, by Mr. Walcot Gibson, F.G.S. This paper, accompanied by a neat little map printed in colours, has now been published. The map shows very clearly the general structure of the great continent, or rather of the considerable portion of it of which we have information that can be represented on a geological map. There is yet a large area extending from British Bechuanaland northwards through the Congo State and to the south of the Libyan Desert, which for the present remains blank, but which ere long will be made more accessible by the railway in course of construction from Mombasa to Uganda. Roughly speaking, however, we still know nothing (or next to nothing) of the geology of a tract almost equal in size to the whole of Europe. That the Geological Record is well represented in Africa, is evident from the full Table of sedimentary and eruptive rocks; but as the author remarks, "by far the larger portion of the continent as yet known is composed of metamorphic crystalline rocks." These rocks show indications of containing many valuable and precious minerals; they are not so barren as would appear at first sight, but our knowledge of them is at present very scanty. The crystalline schists appear to be richest in the territory of the Chartered Company. Newer than this metamorphic series is the famous Banket formation of South Africa; its precise age, however, has yet to be determined. Nor is it possible as yet to state definitely the true sequence on the Rand, for, as the author has himself shown, there is much overthrusting of the strata.

Attention is directed to the various beds of coal, to the possibility of asphalt-deposits being found in East Central Africa, and to sundry other matters of practical interest. The general structure of the continent and the leading facts relating to its geology are clearly pointed out, and the author gives a useful list of the leading works on African geology. He rightly urges that "sooner or later a mineral survey of South Africa will have to be seriously undertaken, in order to arrive at some approximation as to the extent and distribution of the mineral-bearing districts. South Africa and this country would have everything to gain by a Government Geological Survey, as such a survey, being made independently of any personal interest, would tend to reassure the investing public and convince it that the mineral wealth of South Africa is very great."

REPORTS AND PROCEEDINGS.

Geological Society of London.

I.—Annual General Meeting.—February 19, 1897.

Dr. Henry Hicks, F.R.S., President, in the Chair.

The Secretaries read the Reports of the Council and of the Library and Museum Committee for the year 1896. In the former the Council referred to the uninterrupted financial prosperity of the Society, and announced that, for the first time in a period of five years, there had been an increase in the number of Fellows.

During 1896 the number of Fellows elected was 62: of these 48 qualified before the end of the year, making, with 11 previously elected Fellows, a total accession of 59 in the course of the twelvemonth. During the same lapse of time, the losses by death, resignation, and removal amounted to 47, the increase in the number of Fellows being 12. The total number of Fellows, Foreign Members, and Foreign Correspondents stood at 1329 on December 31, 1896, as compared with 1318 at the end of 1895.

The balance-sheet for the year 1896 showed receipts to the amount of £3728 0*s.* 5*d.* (including a balance of £851 15*s.* 5*d.* brought forward from the preceding year), and an expenditure of £2959 17*s.* 5*d.* There was an actual excess of expenditure over current receipts of £83 12*s.* 5*d.*, but the excess was entirely due to expenditure of a non-recurring character, and there still remained at the end of 1896 a balance of £768 3*s.* available for the extraordinary expenditure contemplated in the estimates submitted to the Fellows.

The completion of vol. lii of the Society's Quarterly Journal was announced, as also the publication of No. 3 of the Record of Geological Literature added to the Society's Library, and of Part I of the General Index to the first fifty volumes of the Quarterly Journal. It is hoped to issue to the Fellows the second part of the Index within the next six months.

The institution of a Fund, to be entitled the Geological Relief Fund, of which the Council will be trustees, was announced. It is proposed that the interest accruing from this fund should, from time to time, be applied by the Council in aid of deserving Fellows. The response to the appeal for subscriptions has not, so far, been quite as general as the Council had ventured to hope.

The question as to the desirability of transferring the Society's Collections to the Trustees of the British Museum had been brought up for consideration at a special general meeting on May 20 last, and, after a prolonged discussion, the previous question had been carried by 35 ayes to 12 noes.

In conclusion the awards of the various Medals and Proceeds of Donation Funds in the gift of the Society were announced.

The Report of the Library and Museum Committee enumerated the large additions made during the past year to the Society's Library, the most important of which was the presentation by

Lady Prestwich of 260 bound volumes of tracts, in fulfilment of the wishes of the late Sir Joseph Prestwich.

In presenting the Wollaston Medal to W. H. Hudleston, Esq., M.A., F.R.S., F.L.S., the President addressed him as follows:—

Mr. Hudleston,—

It is extremely gratifying to me that it has fallen to my lot to present to you, on behalf of the Council of the Geological Society, the highest award which it is in their power to bestow on distinguished geologists. You have laboured long and assiduously in the cause of science, and have enriched geological knowledge in its chemical, mineralogical, palæontological, and stratigraphical departments. The first paper which you communicated to this Society appears to be that "On the Chemical Analysis of the Cambrian Rocks," which you were good enough to undertake at my request in the year 1875. You had previously communicated papers to the Geologists' Association—one on the Yorkshire Oolites, and another, in conjunction with Mr. Hilton Price, on the Thames Valley Deposits. Another valuable chemical paper should be referred to, namely, "On the Chemical Composition of the Rocks of the Lizard," which appeared as an Appendix to a paper by Professor Bonney in 1877; and in that year also you, in conjunction with Professor Blake, contributed a highly important paper to this Society on the Corallian Rocks of England. The Addresses delivered by you as President of the Geologists' Association, of the Devonshire Association for the Advancement of Science, Literature, and Art, and of this Society, show much critical power and demonstrate your ability to convey a great amount of valuable information in a limited space. You are further to be congratulated upon having recently completed the important memoir published by the Palæontographical Society—"A Monograph on the Inferior Oolite Gasteropoda"—which contains no less than 514 pages and 44 quarto plates. The great value of your work amongst the Jurassic rocks is so generally admitted that I need not refer to the other papers contributed by you to various Societies.

In conclusion, I can only say that it gives me very much pleasure to hand to you this Medal, which is presented by the Council as a token of their recognition of the eminent services rendered by you to Geological Science.

Mr. Hudleston, in reply, said:—Mr. President,—

On this occasion my warmest thanks are due to the Council, on whom the duty of awarding the Wollaston Medal has this year devolved, for their generous interpretation of my past geological career. To have received a distinction such as the foremost geologists of any country might be proud to accept, and to be enrolled in that famous band of recipients which is headed by the Father of English Geology, is indeed to me an unexpected honour and gratification. If anything were needed to enhance the pleasure of the moment, it is to be found in the circumstance that I receive the Medal from the hands of an old friend and contemporary, whose scientific career possesses so many points of resemblance with my own. For, if I have done some work in Mesozoic geology, you, sir, are well known, both in Europe and in America, for your researches among the older Palæozoic rocks.

This particular award serves to show that the researches concerning the mineral structure of the earth are not necessarily limited to professional enterprise, but that both the Council and the Society are ready to recognize the utility of independent work, whether special or of a more general character. Our science has its ramifications in so many others, that, whether we consider Geology in its tectonic, its petrographical, or its palæontological aspect, it is incumbent upon those who cultivate it to endeavour to acquire, to some extent, a knowledge of cognate subjects. Not that I would venture to lay claim to the accomplishment of much original work in any of the above departments; although I may have contributed something towards a knowledge of the stratigraphy and palæontology of the Jurassic rocks of England, and more especially of my native county.

During the last quarter of a century the Geological Society and the Geologists' Association have, each of them, afforded me a congenial scientific home. If in that interval it has been my good fortune to have furthered the cause which we all have at heart, such service has been more than repaid by the benefits derived from mutual intercourse. Nor do I forget the advantages afforded by my relations with the Palæontographical Society, whose resources have been useful to more than one Wollaston medallist.

In offering my thanks once more to the Council and also to the Fellows here present for the cordiality with which the Award has been received, I may truly say that this Medal will always serve as an agreeable memento of my connection with the Geological Society. So long as I live, it will give me pleasure to take an interest in geological questions, and if I cannot promise to participate very actively in the work of the future, I may at least say, as regards my past work, that this recognition of its merits is most ample, and leaves nothing on my part to be desired.

The President then handed the balance of the proceeds of the Wollaston Donation Fund to F. A. Bather, Esq., M.A., addressing him as follows:—Mr. Bather,—

The Council of the Geological Society have voted you the Balance of the Wollaston Fund as an expression of their appreciation of the excellent work in Palæontology which you are carrying on, and with a view to assist you in the original investigations on the Crinoidea upon which you have expended both much labour and capital.

From the time you left the University of Oxford and entered the Geological Department of the British Museum in 1887, you have specially studied this group of fossil organisms; and in a series of some twenty-seven papers you have not only described many interesting Palæozoic and Mesozoic genera, but have proposed a method of classification, based upon the arrangement of the plates, which will be of the greatest assistance to all future workers.

The paper which you published in 1893 under the auspices of the Royal Swedish Academy, Part I, of the Crinoidea of Gothland (4to, 10 plates, pp. 200), is a work which needs only to be seen and studied to be appreciated, and has been most highly spoken of by eminent palæontologists at home and abroad. I trust this recognition by the Council of what you have already done may encourage you to still greater efforts, so that it cannot for long be said that our English Crinoidea still need a monographer.

Mr. Bather replied in the following terms:—Mr. President,—

After serving the British Museum for two years, I decided to use the knowledge thus gained, and the opportunities there offered, for an exhaustive study of the British Fossil Crinoids, a task to which I was further urged by our much-mourned Fellow, Herbert Carpenter. I estimated that the work would take fifteen years. Half that time has now passed and I have dealt with only a few genera from a single formation. The preliminary studies and other duties, to a few of which you, sir, so kindly alluded, have contracted the time at my disposal within bounds that I long to enlarge. It is the more encouraging that my fellow-workers should honour thus highly the little that I have done, for I am not ashamed to say that the thought of these awards has been, and will be,

"a spur to prick the sides of my intent."

The student of science, however, should not think overmuch of contemporary applause. If a palæontologist, he must wrestle with his fossils, nor let them go until they have yielded all their secrets. Laborious investigation, precise description, accurate and detailed drawings: these are indispensable if he is to receive aught but abuse from a posterity even more critical than ourselves. In the continuance of work that strives, however ineffectually, to have this character, the present award will be a material and a welcome aid.

In presenting the Murchison Medal to Horace B. Woodward, Esq., F.R.S., the President addressed him as follows:—Mr. Woodward,—

The Council have this year adjudged you the Murchison Medal, with the sum of ten guineas, and it is peculiarly appropriate that the award should be made to one who has for so many years zealously worked on the Geological Survey which Sir Roderick Murchison, the founder of the medal, so long and ably directed. It may not be generally known that, like your esteemed father, the late Dr. S. P. Woodward, F.G.S. (who was our Sub-curator in 1839), you also commenced your geological career (in November, 1863) as an assistant in the Museum of the Geological Society, at Somerset House. During the past 30 years of your labours as a field-geologist, on the Geological Survey of England and Wales, your experiences have been most varied. From Newton Abbot and other parts of Devon, over East Somerset, the

Bristol Coalfield, and the Mendips, to the geology of Fakenham and the country around Norwich (where your grandfather, Samuel Woodward, the "Norfolk Geologist," laboured so earnestly 70 years ago), to Essex and the neighbouring drift-covered counties; from the Jurassic areas of Britain south of the Humber to the far-distant Jurassic areas of Sutherland and Skye—all these and more have, in turn, claimed your careful attention. Besides your very numerous Survey memoirs, maps, and sections, all prepared with much skill, you have given us a most helpful work, "The Geology of England and Wales." When President of the Norwich Geological Society, the Norfolk Naturalists' Society, and the Geologists' Association, you gave important addresses, and you have contributed numerous separate geological papers, read here and elsewhere. In addition, I must especially refer to the ever-ready help which you afford, in your office at Jermyn Street (as resident geologist), to all those who call upon you for information, help which has rightly earned for you a large circle of grateful friends.

Mr. Woodward, in reply, said:—Mr. President,—

In thanking you, sir, and the Council of this Society, for the high honour which you have now conferred upon me, I cannot help feeling that you have regarded with great generosity whatever I have been able to accomplish. I would pass by the official or professional work. That has always been a pleasure: it has also been a duty. And I would rather believe that you have taken more into consideration the extra-official or amateur work which has been the labour of leisure hours. This much I would like to say, in reference to the second edition of my "Geology of England and Wales," that the impulse which led to its production was the ambition to render some service; and to do this in the way I desired was to act somewhat independently, for I received no encouragement from publishers. That their predictions were fully justified adds considerably to the gratification with which, sir, I now receive this mark of distinction and approval from your hands.

The President then handed the balance of the proceeds of the Murchison Geological Fund to S. S. Buckman, Esq., addressing him as follows:—Mr. Buckman,—

Following in the steps of your father, Professor James Buckman, you have devoted many years to the elucidation of the Palæontology and Geology of the Lower Oolitic rocks of Dorset and neighbouring counties. In palæontology, you have dealt with the Pelecypods, the Brachiopods, and the Ammonites of the Inferior Oolite and Bajocian, the last group especially in the monograph now being published by the Palæontographical Society. Seeing that accurate work in palæontology could not be accomplished without equally detailed stratigraphy, you have investigated, with a minuteness before unattempted for Jurassic rocks, the unravelling of their geological history, and the Society has thus received from your hands an important series of papers, amongst which I may mention those on the Cotteswold, Midford, and Yeovil Sands (1889); on the so-called Upper-Lias Clay of Down Cliffs (1890); on the Bajocian of the Sherborne District: its Relation to Subjacent and Superjacent Strata (1893); and on the Bajocian of the Mid-Cotteswolds (1895). To show their appreciation of the important work which you have already accomplished, and in the hope that you may continue to work on the lines which have yielded results so excellent, the Council have felt much pleasure in awarding you the Murchison Fund.

Mr. Buckman replied as follows:—Mr. President,—

I scarcely know how to thank you for the honour which you have done me in the presentation of this award, and for the far too favourable manner in which you have spoken of such scientific work as I have been able to accomplish. You have very kindly alluded to my stratigraphical work, and have noticed its minuteness. As regards this part of my labours I have a clear conscience: it was undertaken for the purpose of understanding the genealogy of Ammonites, and its minuteness is an absolute necessity thereto. But when I think of the palæontological work to which you have referred, I feel considerable cause for dissatisfaction. Now that some nine yearly portions have appeared in the volumes of the Palæontographical Society, I ought to be approaching the end of my labours, but I find that I am actually farther from that desirable attainment than when I began, while I have far less opportunity

for continuous study. And when I look back at the earlier portion of this work, I am quite dissatisfied with the survey. I must own to serious mistakes, particularly that I had not sufficiently the courage of my own opinions, and that I gave heed to the outcry about new genera and species. That was a sad mistake, but I have amended it in the later parts of my work. In this direction I have, however, far more to perform. How much—or little—praise I may obtain thereby I know not, but the fact that you have kindly given me this award as a recognition of my work encourages me to confidently pursue my way. It is for such encouragement that I most appreciate this award; and I thank you sincerely for it.

In presenting the Lyell Medal to Dr. George Jennings Hinde, F.R.S., the President addressed him in the following terms:—Dr. Hinde,—

The Council of the Geological Society have awarded to you the Lyell Medal, with the sum of twenty-five pounds, in recognition of your valuable researches in palæontology and geology, but more especially in reference to your discoveries of Fossil Sponges and other minute bodies preserved in cherts, in various formations, and the painstaking manner in which they have been elucidated by you. The experience you gained when working as a student under Professor H. A. Nicholson, in the University of Toronto, and later, under Professor K. A. von Zittel, in the University of Munich (where you obtained your degree of Ph.D., for a dissertation on the Fossil Sponge-Spicules from the Chalk of Norfolk), was an excellent beginning for your subsequent more ripened work. I need only refer to your memoirs on Conodonts from the Cambro-Silurian and Devonian rocks of North America, Scotland, and the West of England, and to your various papers, to show the great value of the original work which you have done. In your "Catalogue of the Fossil Sponges in the British Museum," and in your memoir on "British Fossil Sponges," in the Palæontographical Society, you have given us works of considerable importance. You have also published many other valuable papers which have added much to our knowledge, and all recognize that you have placed yourself in the foremost rank amongst those who have devoted themselves to the study of minute fossil organisms. The medal could not be more worthily bestowed than upon one who has always so earnestly laboured for the advancement of truth, and I have very great pleasure in handing it to you.

Dr. Hinde, in reply, said:—Mr. President,—

It gives me sincere gratification to receive at your hands the Lyell Medal, remembering that it is intended, in the words of its liberal-minded founder, as a mark of honorary distinction and as an expression that the recipient "has deserved well of the science." That so competent a tribunal as the Council of the Geological Society regards my palæontological work as meriting this recognition, is to me a source of lively satisfaction.

I can only regret that so much of the work which it is my aim to accomplish still remains to be done: the encouragement which you have given me to persevere will not, I hope, be without result, but whilst the field of investigation is ever widening and the materials are constantly accumulating, the capacity to keep level with the work becomes, with the lapse of time, a diminishing quantity. I wish here gratefully to acknowledge the large measure of help which has been freely given to me in the course of my work by my brother geologists and on the part of the Society, and more particularly my indebtedness to my friend and indefatigable colleague, Mr. Howard Fox, in working out our joint paper on the Radiolarian Rocks of Devon. For the kindly, sympathetic, and very generous terms in which you, sir, have referred to what I have done, I desire to express my warmest thanks.

The President then handed a moiety of the balance of the proceeds of the Lyell Geological Fund to W. J. Lewis Abbott, Esq., addressing him as follows:—Mr. Abbott,—

Some twenty years ago you read an important paper on the Formation of Agates before the Geologists' Association, and since then you have contributed much additional valuable information in regard to their origin. For many years you have also been a careful and energetic collector of fossil remains, and of the implements worked by early man; and the remarkable collection shown by you

at the recent conversazione in the rooms of the Society testifies to your keen and accurate discrimination. Your researches, as shown in your paper on the Ossiferous Fissures in the Valley of the Shode near Ightham, published in the Quarterly Journal of the Society in 1894, and in other papers published in the Proceedings of the Geologists' Association, the Journal of the Anthropological Institute, and Natural Science, have greatly added to the faunas, and therefore to our knowledge of the history, of the Pleistocene and early Post-Pleistocene periods.

The Council in making this award desire not only to express to you their appreciation of the work which you have already done, but hope that it may be an incentive to further researches. It gives me much pleasure to place this award in your hands.

Mr. Abbott replied as follows:—Mr. President,—

I am deeply grateful to you, sir, and the Council of the Geological Society collectively, for the great honour conferred upon me in thus appreciating the results of my labours; and to you, sir, personally for the very kind, appreciative words with which you have supplemented it. There is, doubtless, a great reward in store for all those who in any way contribute to the stock of human knowledge—a reward which comes with the discovery of anything new. But, after all, there is something which is perhaps even greater to frailly constituted humanity than that: it is to receive such tangible proofs as this that others admit the value of one's labours. It comes as a potent antidote for weeks and months of almost barren research, a grateful compensation for midnight oil expended and pecuniary losses sustained, and a wholesome incentive to renewed energy in the future. I am all the more proud of the honour conferred upon me, in that it is associated with the name of the great ambassador of "the causes now in operation": for assuredly, if there be one branch of geology more than another calculated to make one realize the applicability of these, it is that in which I have been chiefly engaged, where we stand one foot on to-day and the other on yesterday, and note, with equal distinctness and certainty in each, the effects not only of sea and storm, but of the very zephyrs. But while my labours are thus rewarded, it must ever be remembered how much the value of my Pleistocene work has been enhanced by the co-operation of my esteemed colleague Mr. E. T. Newton, and I gladly welcome this opportunity of publicly expressing my deepest obligations and warmest thanks to him for his great and kindly assistance during the last sixteen or eighteen years. In conclusion, I can only express the hope that the remaining years of my life, stimulated anew by this award, will be spent in the cause which I have so much at heart, and that the result of my labours may continue not only to give me pleasure, but prove interesting and profitable to others.

The President then handed the other moiety of the balance of the proceeds of the Lyell Geological Fund to Joseph Lomas, Esq., addressing him as follows:—Mr. Lomas,—

The Council of this Society have this year awarded to you a moiety of the proceeds of the Lyell Fund in testimony of the value of your work, especially in regard to the glacial geology of the neighbourhood of Liverpool, and of areas in North Wales, upon which you have written no less than ten papers between the years 1886 and 1896. To enable you to check the accuracy of your conclusions you have also made a study of glaciation in Switzerland and the Faeröe. As showing that you have not neglected other branches of geology, I may mention also your paper on the Basaltic Dykes of Mull in 1887, and on Fossil Plants from the Carboniferous in 1895. Your recent election as President of the Liverpool Geological Society testifies to the esteem in which you are held by your fellow-workers in that city, where as a special Lecturer on Geology at University College you are responsible for most important geological teaching. It is now my privilege to be the means of handing you this award, with the hope that it may aid you in your further researches.

Mr. Lomas replied in the following terms:—Mr. President,—

You have expressed, in much too kind words, your appreciation of my work in Glacial Geology. Recognizing that the first duty of a student is to collect accurate data, I have done my best to observe and record the phenomena in my own district. If these observations have contributed, even in a small degree, towards the elucidation of a very difficult problem, I am amply rewarded.

We, of the Liverpool Geological Society, are proud to think that four of our Presidents have been honoured by marks of distinction bestowed on them by the mother Society. The encouragement which these awards have afforded cannot be overestimated. When I look down a list of those who have received this award in former years, I cannot but think that the stimulus given has greatly influenced their subsequent careers. I trust that may be so in my own case, and I shall always endeavour to prove that your kindness has not been misplaced.

In presenting the Bigsby Medal to Clement Reid, Esq., F.L.S., the President addressed him as follows:—Mr. Reid,—

The Council of the Geological Society have awarded to you the Bigsby Medal in recognition of the excellent work which you have already accomplished, and in full confidence that it will encourage you to continue those researches with unabated vigour. Since 1874, when you joined the Geological Survey, you have been engaged in mapping many areas and various formations, but you have more especially directed your attention to Tertiary and Pleistocene geology, and your memoir, "On the Geology of the Country around Cromer," has become a classical work of reference. The painstaking manner in which you have searched for evidences of plant-remains from the Pleistocene deposits merits the thanks of all, for the results have been truly remarkable and have enabled us to realize far more clearly than was previously possible the prevailing climatic conditions when they were accumulated. Besides the Survey memoirs which you have written, you have found time to contribute several important communications to this Society and valuable special notes to the papers of other authors. I feel much pleasure in being privileged to hand to you, on behalf of the Council, this medal; and I may be allowed to express the hope that you will continue with the same energy that has hitherto characterized you to add to our knowledge of those special branches of the science which you have already cultivated with so much success.

Mr. Reid, in reply, said:—Mr. President,—

The bestowal of this award, for which I tender my warmest thanks to the Council, makes me feel that perhaps, after all, the results of my work may not be so valueless as I have sometimes feared. No doubt, compared with the magnitude of the problems which I have had before me, the results are very small; and, when viewed as a whole, must seem disconnected. I may say, however, that the published observations have more in common than would appear at first sight. They are the outcome of a continued attack, from different sides, of some of those problems, on the correct solution of which geological progress so largely depends. I allude to the question of the alternations of climate which have taken place in bygone times; the relation of these alternations to the migration, extinction, and variation of species; and to past changes in physical geography; and lastly, to the question of the rapidity with which such changes can succeed one another. In short, my somewhat ambitious task has been that of seeking a base-line for the measurement of geological time.

Towards the attainment of this end I have made but small progress; yet, while following unaccustomed paths noteworthy facts are constantly discovered, and many a line of research, almost a failure from the point of view of the original inquiry, has yet repaid the time and labour bestowed upon it. It is only, I am afraid, on such minor questions that I have, as yet, been able to throw any light. My problem still lies before me.

The President then proceeded to read his Anniversary Address, in which he first gave obituary notices of several Fellows and Foreign Members deceased since the last annual meeting, including Auguste Daubrée (elected F. M. in 1867), H. E. Beyrich (elected F. M. in 1876), J. D. Whitney (elected F. M. in 1887), Baron C. von Ettingshausen (elected F. C in 1884), Sir J. Prestwich (elected a Fellow in 1833, President in 1870–1872), A. H. Green (elected in 1862), Capt. Marshall Hall (elected in 1866), W. Sharp (elected in 1840), H. J. Slack (elected in 1849), R. N. Worth (elected in 1875), and D. Robertson (elected in 1877).

He then dealt with the recent evidence bearing on the Geological and Biological History of early Cambrian and Pre-Cambrian Times, pointing out that very different views are now held in regard to the origin of many of the Pre-Cambrian rocks from those which prevailed from twenty to thirty years ago. Then all the crystalline schists and gneisses, and many also of the granitic rocks, were looked upon as metamorphosed sediments. Gradually, and mainly from the careful microscopical work which has been carried on by so many eminent petrologists in this and other countries, we have been led to recognize that a very large proportion of the crystalline rocks previously supposed to have been sediments are igneous rocks which have been crushed, cleaved, and have suffered other changes from chemical and mechanical influences. It has been shown also that very large and unexpected proportions of the Pre-Cambrian rocks were built up from materials derived from volcanic outbursts, and that this was particularly the case in the British areas. As a consequence, few of the sediments are such as could have been deposited in marine areas favourable to organic life, or are such as would be likely to retain very definite evidence of its existence. Still, the very earliest of the Cambrian rocks, or those sediments which were deposited in a fairly tranquil sea, following the great physical changes which took place at the close of the Pre-Cambrian era, contain ample evidence that the sea which was gradually encroaching on the Pre-Cambrian land on both sides of the Atlantic teemed with life, in which probably all the orders of the Invertebrata were represented.

These discoveries had undoubtedly added most important evidence in support of the theory of the gradual evolution of organic life, and led one to speak very hopefully of finding yet earlier traces of the life-history of our globe. The readiness with which suitable forms tenanted all the areas as they were covered by the sea showed that there was abundance of life near at hand in the adjoining submerged areas, and that the changes then taking place were only such as had been going on more or less continuously on the surface of the globe from the moment when the waters began to collect or, at any rate, became suitable for the existence of life. The earliest records may have been completely blotted out by the repeated changes which had previously taken place, or may be entirely buried beneath and hidden from our view in the great oceanic areas. Still, the results obtained within comparatively recent years led one to hope that there is yet room to discover much in those areas which hitherto have been but imperfectly examined or not at all explored.

The President concluded that as geologists we feel quite certain that from the earliest Cambrian to the present time we are dealing only with a comparatively recent period in the world's history; and, as biologists, that we are convinced that innumerable successions of organic beings must have lived and passed away in the Pre-Cambrian era before such an advance could have been attained, as is manifest in those forms which tenanted the earliest Cambrian

seas. Recent researches had enabled geologists and palæontologists to take up a far more satisfactory position in regard to the theory of evolution than when Darwin felt constrained to pen the following words, when referring to the imperfection of the geological record ("Origin of Species," p. 310):—"The several difficulties here discussed, namely, our not finding in the successive formations infinitely numerous transitional links between the many species which now exist or have existed; the sudden manner in which whole groups of species appear in our European formations; the almost entire absence, as at present known, of fossiliferous formations beneath the Silurian strata, are all undoubtedly of the gravest nature. We see this in the plainest manner by the fact that all the most eminent palæontologists, namely, Cuvier, Owen, Agassiz, Barrande, Falconer, E. Forbes, etc., and all our greatest geologists, as Lyell, Murchison, Sedgwick, etc., have unanimously, often vehemently, maintained the immutability of species."

The ballot for the Council and officers was taken, and the following were declared duly elected for the ensuing year:—*Council*: H. Bauerman, Esq.; W. T. Blanford, LL.D., F.R.S.; Prof. T. G. Bonney, D.Sc., LL.D., F.R.S.; Prof. W. Boyd Dawkins, M.A., F.R.S.; Sir John Evans, K.C.B., D.C.L., LL.D., F.R.S.; F. W. Harmer, Esq.; R. S. Herries, Esq., M.A.; Henry Hicks, M.D., F.R.S.; Rev. Edwin Hill, M.A.; Prof. E. Hull, M.A., LL.D., F.R.S.; Prof. J. W. Judd, C.B., LL.D., F.R.S.; R. Lydekker, Esq., B.A., F.R.S.; Lieut.-General C. A. McMahon; J. E. Marr, Esq., M.A., F.R.S.; Prof. H. A. Miers, M.A., F.R.S.; H. W. Monckton, Esq., F.L.S.; E. T. Newton, Esq., F.R.S.; A. Strahan, Esq., M.A.; J. J. H. Teall, Esq., M.A., F.R.S.; W. W. Watts, Esq., M.A.; W. Whitaker, Esq., B.A., F.R.S.; Rev. H. H. Winwood, M.A.; H. Woodward, LL.D., F.R.S. *Officers*:—*President*: Henry Hicks, M.D., F.R.S. *Vice-Presidents*: Prof. T. G. Bonney, D.Sc., LL.D., F.R.S.; Lieut.-General C. A. McMahon; J. J. H. Teall, Esq., M.A., F.R.S.; H. Woodward, LL.D., F.R.S. *Secretaries*: J. E. Marr, Esq., M.A., F.R.S.; R. S. Herries, Esq., M.A. *Foreign Secretary*: Sir John Evans, K.C.B., D.C.L., LL.D., F.R.S. *Treasurer*: W. T. Blanford, LL.D., F.R.S.

II.—March 10, 1897.—Dr. Henry Hicks, F.R.S., President, in the Chair. The following communications were read:—

1. "Volcanic Activity in Central America in relation to British Earthquakes." By A. Gosling, Esq., H.M. Minister and Consul-General in Central America. Communicated (through H.M. Foreign Office) by the President.

The author of the communication points out that the volcano of Izalco, in the Republic of Salvador, which has been in active eruption for over one hundred years, suddenly ceased to be so within a fortnight of the period at which the communication was sent (December 20, 1896), and he notes the occurrence of seven shocks of earthquake in England on December 17, 1896. He quotes remarks concerning the volcano, which were contributed by him to the "North American Review" in January, 1896.

2. "The Red Rocks near Bonmahon on the Coast of Co. Waterford." By F. R. C. Reed, Esq., M.A., F.G.S.

The rocks which are considered in this paper have been regarded by some authorities as deposits interstratified with the Lower Palæozoic rocks of the district, while others have maintained that

they are of Old Red Sandstone age. It is the object of the author to show the correctness of the latter supposition, and he brings forward evidence to prove that the red rocks rest unconformably upon the Lower Palæozoic rocks, or are faulted against them, and that the breccias of the red rocks contain fragments of the Lower Palæozoic rocks, and also of intrusive rocks which break through the latter. The red rocks also resemble deposits which are known to be of Old Red Sandstone age.

The Old Red Sandstone rocks of the district form an irregular and incomplete elliptical ring around a denuded plateau of older rocks. The incompleteness is due to the concealment of the southern part of the ring beneath the sea; but if the southern part of this ring be as irregular as the northern portion, faulted patches of the Old Red Sandstone rocks may well come in among the older rocks of the cliffs in the positions where the beds which are discussed in this paper occur.

3. "On the Depth of the Source of Lava." By J. Logan Lobley, Esq., F.G.S.

The author contends that lava cannot have been brought to the surface from a depth of 30 miles, as fissures which would serve as conduits could not exist at that depth, and, moreover, the lava would be consolidated before it reached the surface, owing to contact with cool rock for a considerable period. He argues that the pressure of the overlying rocks would cause the rocks even at a depth of 10 miles to be practically plastic, as shown by M. Tresca's experiments, and that no continuous fissure could occur in such rocks. Estimates of the volumes of ascending lava-columns were given, with a diagram comparing them with a 30-mile thickness of rocks.

CORRESPONDENCE.

THE HIGH-LEVEL SHELLY CLAYS AND MR. MELLARD READE.[1]

SIR,—My "writings on the subject" are not so "numerous" by a long way as those of Mr. Mellard Reade, but I had hoped that they were at least tolerably clear as far as they went. I find, however, that my friend Mr. Reade has "completely misunderstood their tenour." He thinks I have made "strenuous endeavours to prove that high-level shelly beds do not exist." This would indeed be a waste of time on my part, in all cases where they were ordinarily well attested. No; freely admitting them in all such cases, the point of interest with me has been—How were they formed or deposited? Are they *in sitû*, and do they indicate former levels of the sea? Mr. Reade seems to hold as a matter of course that they are and do; and he even suggests that if such shelly clays are found in one place, say at 1000 feet, it becomes "futile" and a "waste of time" to question whether a different bed (from all description) is found in another place at 500 feet! On the contrary, I hold that as some of these clays are known to

[1] GEOL. MAG., March, 1897.

have been transported, more of them than we imagine may be so; that therefore they do not necessarily indicate former levels of the sea; and that every case is to be judged of by its own evidence.

As regards Chapelhall, I certainly pointed out the slender evidence on which it rested, it being, so far as I know, quite exceptional in this respect; but I may say, on behalf of all the Committee, that it was a disappointment to them that the shelly clay was not found; for it was hoped that a careful examination of it would throw light on its origin and mode of formation. This would have been much more satisfactory to them than not finding it at all.

But Mr. Mellard Reade makes a far more important mistake than any regarding the purport of my poor papers. Referring to the suggestion that the Ayrshire beds have been laid down by a Frith of Clyde glacier, he says—"I can only point out that the hypothetical course of such a glacier does not correspond with that of any map I have yet seen which professes to give the lines of glacial flow in Scotland." Now, this is a point which should be easily settled. I suppose that the two best, most recent, and most authoritative maps of the ice-flow in Scotland are those by the Messrs. Geikie—Sir Archibald's sketch-map in his "Scenery of Scotland," 2nd edition, p. 248; and Professor James Geikie's of the British Isles in his "Great Ice Age," 3rd edition, p. 69. Both of these maps distinctly show the lines of ice-flow extending from the mountainous region around the heads of Loch Lomond, Loch Long, etc., across the opening of the Frith of Clyde, over the low grounds of Renfrewshire and Ayrshire, and curving out to sea in the neighbourhood of Ayr, as the West Highland ice came into contact with the ice from the Southern Uplands. And Sir Archibald Geikie expressly says, referring to the striations along the estuary of the Clyde—"These markings prove that the mass of ice moved southward from Loch Lomond, crossed the Clyde, passed over the hills of Renfrewshire, and crept down into the heart of Ayrshire, where it united with the ice that was streaming northward from the Southern Uplands" ("Scenery of Scotland," 2nd edition, p. 246). Professor James Geikie also notices the trend of the ice-markings in the lower reaches of the Clyde in similar terms "(Great Ice Age," 3rd edition, pp. 69, 70).

Now, will Mr. Mellard Reade kindly say what maps he has seen, "professing to give the lines of glacial flow in Scotland," which show them differently from the above? We may then come to understand how he does not appear to have seen Messrs. Geikies' maps, nor I those to which he refers.

GLASGOW, *March* 8, 1897. DUGALD BELL.

CYCADEOIDEA GIGANTEA, SEWARD: AN OMISSION.

SIR,—May I make use of your Magazine for the purpose of pointing out an unfortunate omission in a recent paper on a new Cycadean stem from Portland. In the last number of the Quarterly Journal of the Geological Society I gave a description of the

magnificent specimen of *Cycadeoidea*, which is now in the British Museum Fossil Plant Gallery.

Since this was published my attention has been called to the fact that it is to Mr. A. M. Wallis, guide and quarryman, of Portland, that we are primarily indebted for this valuable addition to the National Collection. Mr. Wallis, on discovering the specimen, at once realized its scientific value, and took care that it fell into the hands of those who appreciated its worth. I am much indebted to Mr. Strahan for calling my attention to this omission, and for furnishing me with the above facts. A. C. Seward.

Cambridge, *March* 11, 1897.

THE AGE OF THE MORTE SLATES.

Sir,—If Dr. Hicks had only claimed that the Morte Slates are older than the Ilfracombe Beds, probably no palæontologist would have objected on the evidence at present available. But Dr. Hicks claimed that the fossils proved the Morte Slates to be Silurian.

If Dr. Hicks considers the Oriskany Sandstone to be Silurian, his views are as reactionary regarding the Devonian rocks of America as they are revolutionary regarding those of England.

In reference to the suggestion that I had not noticed that the specimen shown in figure 3 in the text did not belong to the same individual as the specimen shown in pl. x, fig. 6, I would ask whether the fossil fox on the staircase of the Geological Society and the fossil fox in the British Museum are to be called one specimen? I should think most people would call them two specimens representing one individual.

I am grateful to Dr. Hicks for emphasizing my argument that *Modiolopsis* cannot be recognized without seeing the dentition. But as this is not shown in any of the Morte Slate specimens, why were they referred to the typically Silurian genus *Modiolopsis*?

In regard to the identification of the big Brachiopod, I did not refer to the "characteristic ribbing," as I did not think the character worth referring to. I have nothing to add or retract from my remarks about this fossil, and prefer to leave the question to the ultimate decision of palæontologists. J. W. Gregory.

OBITUARY.

CHARLES TOMLINSON, F.R.S.

Born in 1808. Died February 15, 1897.

By the death of Charles Tomlinson science has lost a man of great learning, and who may perhaps be appropriately described as a Natural Philosopher of the old school. Although best known for his researches on physical and chemical subjects, as Lecturer on Experimental Science at King's College, and as one of the founders of the Physical Society; he was also a man of great literary attainments, and one who had given a good deal of attention to subjects of Natural History, including Geology.

He joined the Geologists' Association soon after it was established in 1859, and was for several years an active member, serving on

the "General Committee" in 1862. During this period he communicated several papers: (1) "On the Action of Heat on certain Sandstones of Yorkshire" (Proc. Geol. Assoc., vol. i, p. 50), when he drew attention to the road-metal used near Huddersfield, and to the custom of hardening the stone by the action of fire; (2) "On the Efflorescence which succeeds the action of Heat on certain Sandstones of Yorkshire" (ibid., p. 158); (3) "On the Plasticity and Odour of Clay" (ibid., p. 237), in which he observed that clay ceased to be plastic when deprived of its chemically combined water, and that plasticity and other properties depend upon variations in the attractive force of the molecules of matter according to the distances apart of such molecules. The peculiar odour of clay belonged only to impure clays, and chiefly to those containing oxide of iron. H. B. W.

THE REV. JOHN EDWARD CROSS., M.A., F.G.S.

(Prebendary of Lincoln).

Born 1821. Died February 28, 1897.

The Rev. J. E. Cross (brother of Viscount Cross) was educated at Christ Church, Oxford, and was appointed to the curacy of Bolton-le-Moors in 1846. Three years later he became curate of Appleby, near Brigg, in Lincolnshire, and he was presented to the vicarage in 1856. In this quiet village he lived and laboured for thirty-five years, retiring in 1891 to Grange-over-Sands.

To geologists Mr. Cross is known as the author of an excellent paper on "The Geology of North-West Lincolnshire" (Quart. Journ. Geol. Soc., vol. xxxi, pp. 115–130. Appendix by R. Etheridge). In this work he described, for the first time in detail, the Jurassic formations from the Lower Lias to the Cornbrash, in the area bordering the south shore of the Humber between the rivers Trent and Ancholme. It was, as he remarked, "a corner of the land unknown to fame" until the discovery of the valuable iron-ore in the Lower Lias at Frodingham. Long and diligently had Mr. Cross worked, as shown by his careful record of facts, and the numerous fossils he had collected. Several new species of Mollusca were described by Mr. Etheridge. H. B. W.

MISCELLANEOUS.

New Geological Survey Maps.—Attention was drawn in the Geological Magazine for February, 1896, p. 96, to the General Map of England and Wales (on the scale of four-miles to one inch), which was in course of publication by the Geological Survey. We now learn that the hand-coloured edition of this useful map is complete, the fifteen sheets, including title and index of colours, all being published. We called attention to the fact that Sheet 12 of this map (London Basin and Wealden area) was issued last year at the greatly reduced price of 2*s.* 6*d.*, *printed in colours*. We are glad to announce that three other sheets have now been similarly published, viz.: Sheet 6 (East Yorkshire), Sheet 9 (Eastern Counties), and Sheet 15 (Sussex Coast). The price is uniformly 2*s.* 6*d.*